Money, Self-focus, and Politics

A Critical Examination of Money Priming, Its Mechanisms, and Its Effects on Political Attitudes

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Im Einklang mit §9 Abs. 1c) der Promotionsordnung der Fakultät für Sozialwissenschaften der Universität Mannheim bestätige ich, Johannes Schuler, mit meiner Unterschrift, dass alle angeführten Manuskripte in Zusammenarbeit mit den jeweiligen Koautoren von mir konzipiert, vorbereitet und angefertigt worden sind. Persönliche Erklärungen zu welchen Anteilen die Beiträge in eigener Verantwortung selbstständig geleistet wurden, sowie eine Bestätigung dieser Erklärung durch die Ko-Autoren Michaela Wänke, Tobias Vogel, Igor Ivanov, Emiel Cracco und Marcel Brass gemäß §9 Abs. 1c) der Promotionsordnung sind im Anhang separat beigefügt.

Nach §9 Abs. 1a) bestätige ich die Übereinstimmung der schriftlichen mit der elektronischen Fassung der Dissertation.

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Johannes Schuler, Mannheim den

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I am not a big fan of lengthy personal statements and praise for such an ordinary accomplishment, which thousands of other PhD students in Germany do every year. However, it would not be appropriate (and most likely impolite) to skip this part, because this dissertation would not have been possible without the help and support of many persons in my professional and social environment, which is why I briefly want to acknowledge their support at this point

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Overview

This paper-based dissertation is structured in two parts. A synopsis provides a theoretical frame, summarizes the empirical findings, and connects and embeds them into the literature. The second part consists of the empirical findings reported in four articles as appendices A to D. Because the articles were written for publication and changed due to various suggestions of reviewers and editors, each of the articles form independent reports that may have content-related redundancies (overlaps) and excursions that might not directly relate to the topic of this dissertation. Especially our third report (Appendix C) was written to a broad audience and with a broader implication and therefore entails examples from other fields and a large theoretical part.

The synopsis starts with an overview on priming in social psychology in order to point out the historical development of the field and the challenges that it faces nowadays. This overview is intended to demonstrate the general state of the field and to show that money priming - as one field of priming in social psychology – faces similar challenges as the field in general. The overview on priming is followed by an overview on money priming, its proposed underlying mechanisms and recent critiques. I then follow with a description of some initial studies (not reported in greater detail) to show how this led me to my research program. To introduce my research I also explain in more detail the research by Caruso, Vohs, Baxter, and Waytz (2013), because the incoherencies of this paper inspired the research that resulted in my three subsequent articles (Appendix A-C). A description of the key findings of the studies (see below) together with some comments, conclusions, and implications for the field that did not necessarily make their way into the respective articles, form the main part of this synopsis. I end with a general discussion that connects the individual findings and articulates implications of this research for the field.

The second part consists of eleven studies that are reported in more detail. The first three studies deal with the subjective standing in the social hierarchy as an important moderator for effects of money priming (Appendix A). Then, a meta-analysis of seven studies that tests whether money primes change political views is reported. It reveals tentative evidence that the subjective standing in the social hierarchy moderates this effect as well (Appendix B). Appendix C is a theoretical paper on non-significant replications in which an exploratory study with German psychology students indicates that they tend to overinterpret the evidence of non-significant replications. Appendix D is a preregistered report that entails a pilot study. Here we describe a specific preregistered study, which we propose in order to test whether money primes affect the self-focus of a person or not.

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Introduction

Priming is one of the most controversial and popular fields in Social Psychology. Soon after the pioneering work from Meyer and Schvaneveldt (1971) and even earlier from Karl Lashley (for review see: Bargh, 2014), the field began flowering, produced many astonishing findings (Bargh, Chen, & Burrows, 1996; Bargh, Gollwitzer, Lee-Chai, Barndollar, & Trötschel, 2001; Dijksterhuis & van Knippenberg, 1998; Fazio, Sanbonmatsu, Powell, & Kardes, 1986; Higgins, Rholes, & Jones, 1977; Srull & Wyer, 1979) and quickly became one of the most popular fields in psychology for the popular media (e.g., BBC, 2012; Fehrenbach, 2010; Matter, 2013; Pryor, 2016; Yuhas, 2015), or, as Higgins and Eitam (2014) put it, priming "morphed from being a stagehand to being the star of the show" (p. 225). The last 40 years have shown that subtle environmental cues can activate social norms such as in-group loyalty (Hertel & Kerr, 2001), stereotypes on race and gender (Chen & Bargh, 1997; Shih, Pittinsky, & Ambady, 1999), goals that influence performance and cooperation (Bargh et al., 2001), and concepts that provoke prosocial behavior (Shariff & Norenzayan, 2007).

Nowadays, in a time where Psychology seems to be in a crisis of confidence (Pashler & Wagenmakers, 2012), "priming research has been a particular target for those who care about replication in psychology" (Cesario, 2014, p. 40) and the "field is now the poster child for doubts about the integrity of psychological research" (Kahneman, 2012). This might have several reasons. First, priming research produced a plethora of truly surprising and astonishing findings that probably seem so extraordinary because the manipulation is subtle, and works completely without a person's awareness of this influence. Second, priming research, still being in its infancy, often lacks a clear explanation and knowledge on the psychological mechanisms and limiting factors (Cesario, 2014; Molden, 2014) and therefore the research might require some experience and certain skills to obtain and reproduce priming effects (Kahneman, 2012). Be that as it may, the comparatively young field, which has

developed dramatically in the last decades and which has received a great amount of attention, is today viewed with increasing skepticism regarding the reliability of its findings (Payne, Brown-Iannuzzi, & Loersch, 2016).

In a first step I will give a brief overview on priming in social psychology, the current stage of research and new challenges priming researchers are facing nowadays. Furthermore, I will describe in more detail some of the critique that has been voiced on priming research. This introduction on priming is intended to help the reader understand a subfield that is – with its problems, critiques and theoretical approach – in a way a typical example for the field in general: money priming. Money priming is comparatively young but has already undergone the same types of critiques and challenges as priming research in general. In a subsequent step I will describe my own research and how it connects to the current challenges that priming researchers are faced with.

Priming in Social Psychology

Priming can be described as a facilitating effect of any event or action on a subsequent associated response (Molden, 2014; Tulving, 1983). More specifically it is as an implicit memory effect where the exposure to a stimulus affects a person's response to a subsequent stimulus (Smith & Branscombe, 1987). Priming effects can be found when encountering a stimulus a second time (repetition priming), when a stimulus activates a related concept (semantic priming) or when an associated goal (goal priming) is triggered (Doyen, Klein, Simons, & Cleeremans, 2014). However, within social psychology, research focused primarily on the activation of social representations such as social norms, stereotypes, goals or traits (Molden, 2014). In other words, until today, social psychologists typically want to study how social representations influence real world judgments, beliefs and actions, rather than studying the structure of knowledge representations in the memory (Doyen et al., 2014).

The early work on priming in social psychology focused on the perception of social targets, for example how primes can affect judgment on personalities (Srull & Wyer, 1979), one's own emotions (Sinclair, Hoffman, Mark, Martin, & Pickering, 1994) and the activation of stereotypes (Devine, 1989) to name only a few. However, beginning in the 90's the focus shifted from altering the perception of social targets to altering actual behavior (Molden, 2014) of many kinds (e.g., Bargh et al., 1996; Bargh et al., 2001; Hassin, Ferguson, Shidlovski, & Gross, 2007; Zhong & Liljenquist, 2006). Hence, researchers investigated at this time what types of behavior could be primed and which stimuli could be used to provoke a behavior up to a point where the same prime can provoke a variety of qualitatively different effects that do not even seem to have the same mediating mechanism (Bargh, 2006). Arguably this focus on discovering new effects, which without doubt provoked a plethora of highly diverse findings, also led to negligence for the theoretical framework and the mediating psychological mechanisms underlying these effects.

So it is not surprising that one frequent concern about the field that is raised by priming researchers themselves is its lack of theoretical foundation or in other words, the ability to specifically describe the mediating psychological mechanism that triggers an effect (Cesario, 2014; Higgins & Eitam, 2014; Locke, 2015; Molden, 2014). However, the recent skepticism about the nature, reliability and validity of priming effects (as described in: Bargh, 2014; Cesario, 2014; Kahneman, 2012) did not emerge due to insufficient knowledge about the mediating mechanisms of priming effects, but due to prominent failures to replicate the original effects (e.g., de Molière & Harris, 2016; Donnellan, Lucas, & Cesario, 2015; Gomes & McCullough, 2015; Harris, Coburn, Rohrer, & Pashler, 2013; Klein et al., 2014; Open Science Collaboration, 2015; Pashler, Coburn, & Harris, 2012; Pashler, Rohrer, & Harris, 2013; Shanks et al., 2013; Wortman, Donnellan, & Lucas, 2014). In their well noted replication attempt Doyen, Klein, Pichon, and Cleeremans (2012) not only failed to replicate

Bargh et al.'s (1996) famous elderly priming experiment - which became the flagship experiment for priming research that is cited in every textbook on social psychology - but also provided evidence that the effect might be due to a simple experimenter bias. While many priming researchers argue that unsuccessful replications in priming research should actually be expected, considering the insufficient knowledge about the mechanisms and limiting factors (e.g., Cesario, 2014), it has led others to question priming effects in general (e.g., Harris et al., 2013). Ultimately this general skepticism towards priming effects in social psychology voiced mainly from cognitive psychologists might also be due to a third problem that priming research is facing. Researchers from different backgrounds do not yet fully agree on a clear definition of priming in general. First and foremost, researchers from cognitive and social psychology disagree on whether a prime needs to be processed without awareness or not and which mediating mechanisms can explain the effects (Doyen et al., 2012; Doyen et al., 2014). Furthermore, cognitive psychologists often argue that priming effects last only several seconds and not as proposed in many priming experiments in social psychology, several minutes (e.g., Rohrer, Pashler, & Harris, 2015; cf., Wentura & Rothermund, 2014).

Thus, in light of a lack of theoretical precision for the mechanisms, a lack of robust replicability and a lack of a common ground of priming between different fields[1], priming in social psychology has some challenges – or as framed by Zanna and Fazio (1982) *second- or third-generation questions* – that priming researchers need to tackle. Bargh (2006) listed some of these challenges for the field such as to investigate which role individual differences play in the effectiveness of a prime and how one prime can produce such qualitatively different effects. But probably the most urging challenge in priming research is the lack of replicability in the field (Kahneman, 2012). However, as a general believer in priming

[1] I will not further elaborate on the lack of a common ground between different fields of priming (for reviews see: Doyen et al., 2014; Smith & DeCoster, 2000) but focus on priming research in social psychology.

effects, to my view the replicability problem is mainly a proxy for the need to discover the conditions for *when* (a moderator question) and ultimately *why* (a mechanism question) a phenomenon appears or disappears (Higgins & Eitam, 2014; Zanna & Fazio, 1982). This does neither mean that all priming effects in social psychology are real, nor does it imply that a lack of replicability is not a problem; I simply believe that the replication problem will persist unless we gain a better understanding of the mechanisms and limiting factors.

Priming research in social psychology has already made considerable progress in this regard and discovered some important factors that allow predicting *when* priming effects occur (e.g., Mayo, 2015; Wheeler & Berger, 2007; Wheeler & DeMarree, 2009) and allow explaining *why* and *how* they occur (e.g., Förster, Liberman, & Friedman, 2007; Loersch & Keith Payne, 2016; Wheeler & Petty, 2001). As one example, depending on the extremeness of the prime, assimilation effects (judgments consistent with the primed category) versus contrast effects (judgments opposite to the primed category) were soon discovered in priming research (Herr, Sherman, & Fazio, 1983). Further research helped to identify *why* priming effects occur in the expected direction or not. The subtlety versus blatancy (i.e., awareness) of the prime determines whether participants respond consistent with the primed category or whether they (over)correct for the influence (Newman & Uleman, 1990; Strack, Schwarz, Bless, Kübler, & Wänke, 1993).

Money Priming Research

Understanding the current state of money priming is much easier when considered in the context of priming research in general. Even though this strand of research evolved only ten years ago and only represents a subfacet of priming phenomena in social psychology, the field faces in many ways the same aforementioned challenges such as problems with replicability, unclear psychological mechanisms and general skepticism about the effect.

With their first and seminal findings on money priming Vohs, Mead, and Goode (2006) reasoned that humans have an abstract economic concept of money that can be activated with subtle primes. More specifically the authors claimed that without a person's awareness, subtle money related stimuli such as pictures of money[2] would heighten the accessibility of the proposed concept of money (Vohs et al., 2006; Vohs, Mead, & Goode, 2008). In their theoretical approach, this concept does not include the possession of money but only the mere 'idea' of it, which – at least on a conceptual basis – discriminates the field from research that deals with spending money (Dunn, Aknin, & Norton, 2008) or research that deals with effects of personal wealth and possessions (Johnson & Krueger, 2006; Nelson & Morrison, 2005).[3]

Research has shown that the activation of the concept money can have tremendous effects on people's behavior and attitudes. The two major effects seem to be that money primed participants put less emphasis on social relations and more emphasis on themselves and the achievement of personal needs and goals (Reutner & Wänke, 2013; Vohs et al., 2006; 2008; for review see: Vohs, 2015). To name just a few findings that concur with this general tendency, participants tended to work more persistently on tasks (Boucher & Kofos, 2012; Vohs et al., 2006; Zedelius, Veling, & Aarts, 2013), were less vulnerable for social exclusion (Zhou et al., 2009), and generally focused more on themselves rather than others. This focus manifested in a decreased willingness to help others (Guéguen & Jacob, 2013; Pfeffer & DeVoe, 2009; Roberts & Roberts, 2012; Vohs et al., 2006), a preference for solitary activities

[2] Frequently used money primes are: a sentence descrambling task, seeing money on a screensaver or poster (Vohs et al., 2006), counting and handling money (Zhou, Vohs, & Baumeister, 2009) or subtle visual money cues in the background of the experiment (Hansen, Kutzner, & Wänke, 2013).

[3] In many priming experiments it is however on an operational level not clear whether only the concept of money or reminders of personal or others' wealth are activated (e.g., Gino & Pierce, 2009; Mogilner & Aaker, 2009; Zhou et al., 2009) and whether this actually makes any difference.

and increased social distance (Vohs et al., 2006), and an increased concern for ones own compared to other's benefits (Gąsiorowska & Hełka, 2012; Reutner & Wänke, 2013; Teng, Chen, Poon, Zhang, & Jiang, 2016), even if this required immoral behavior (Gino & Mogilner, 2014; Gino & Pierce, 2009; Kouchaki, Smith-Crowe, Brief, & Sousa, 2013). Thus, while it seems that money primes help to focus and to reach ones personal goals, it is at the same time bad for the social self (Gasiorowska, Zaleskiewicz, & Wygrab, 2012; Vohs et al., 2008) and eventually bad for ones own happiness (Mogilner, 2010; Quoidbach, Dunn, Petrides, & Mikolajczak, 2010).

Vohs et al. (2008) suggest a market pricing orientation (Fiske, 1992) in which actions and choices are assessed in terms of personal inputs and outputs, as the mediating mechanism for their findings. This general focus is complemented by an inner state of *self-sufficiency* "wherein people put forth effort to attain personal goals and prefer to be separate from others" (Vohs et al., 2006, p. 1154) and "shift their focus more toward self-related needs and less toward the needs of other people" (Reutner & Wänke, 2013, p. 220). In consequence, this psychological state leads to a focus on the self and one's personal goals and accordingly increases personal performance and the need for independency while at the same time reduces the will to interact with others, the sensitivity for others needs, and the willingness to act in ways that are not directly advantageous for oneself (Vohs et al., 2008). For critical psychologists, this description might appear vague and superficial (and in fact the psychological mechanism is not described in more detail), but other mechanisms are not much more specific either.

Alternative Underlying Mechanisms

Even though the suggested mechanism can explain a great deal of research, this theoretical framework cannot entirely explain other findings. Thus, many alternative mediating mechanisms have been proposed to explain some of the findings in the field.

Gino and Pierce (2009) argue (and present evidence for their claim) that the increased unethical cheating of participants is induced by feelings of envy (not of self-interest), because the presence of money on an experimenter's table reminds them of other people's (in this case the experimenteer's) wealth. Hansen et al. (2013) proposed that priming subjects with money activates the concept of money as a universal resource (see also: Lea & Webley, 2006; Zhou et al., 2009), which in turn triggers feelings that needs can be met and that potential challenges in the environment can be overcome. Because former research had demonstrated that human cognition adapts to problematic environments with a more detailed oriented approach and a lower construal level whereas save environments foster higher construal levels, the authors showed that the exposure to large quantities of money activates a higher construal level. Caruso et al. (2013) found that participants were more likely to endorse free-market systems and to accept social inequality and social dominance when primed with money. In this case the authors explained their effects on underlying core beliefs of political right-wing orientation with the claim that money serves as a symbol (and a vehicle for the functioning) of free-market capitalism and therefore makes participants more likely to endorse free-market systems and their related concepts of social dominance and just world-beliefs after being reminded of money.

These explanations for money priming effects are still not exhaustive. Other hypothesized mechanisms to explain money priming effects suggested that subtle cues of money provoke a business decision frame (Jiang, Chen, & Wyer Jr, 2014; Kouchaki et al., 2013), tendencies to

obey moral and legal rules (Yang et al., 2013), and an existential anxiety buffer (Zaleskiewicz, Gasiorowska, Kesebir, Luszczynska, & Pyszczynski, 2013).

The remarkable variety of effects and suggested mechanisms does not necessarily question the validity of the findings. As noted, one prime can provoke different effects with different underlying mechanisms (Bargh, 2006). However, while it is certainly possible that the same prime provokes different effects for different reasons, it seems a major challenge to the field (a) to analyze, which of the suggested mechanisms reliably predict one group of effects (i.e., discover which parsimonious explanation can explain a set of effects and discard flawed post-hoc theories) and (b) to identify the factors that cause these different effects.

Skepticism on Money Priming

It is indispensable to mention that in the last few years and during the course of this research, there has been a growing skepticism on money priming. Two articles became suspected (and one convicted) of data fraud (Pashler, Rohrer, Abramson, Wolfson, & Harris, 2016; Retraction 2014) and two large-scale attempts to replicate money priming findings from Caruso et al. (2013) were unsuccessful (Klein et al., 2014; Rohrer et al., 2015). Even more concerning, Rohrer et al. (2015) reported that both Vohs et al. (2006) and Caruso et al. (2013) had conducted additional unreported studies, had used additional dependent variables and mediators that were not reported in their original work, and had no clearly defined stopping rules to determine their sample sizes.

Even though, other conceptual replications of the seminal findings from Vohs et al. (2006) were successful (Capaldi & Zelenski, 2016; Savani, Mead, Stillman, & Vohs, 2016; for overview: Vohs, 2015), the disturbing report by Rohrer and colleagues raised considerable attention and somewhat justified skepticism to the seminal first findings and probably the field in general. Inspired by this replication report and a review that used a vote-counting

approach to support the two major effects of money priming (Vohs, 2015), Vadillo, Hardwicke, and Shanks (2016) statistically examined the presented evidence. The authors found with different meta-analytic tools that the original studies in two papers (Caruso et al., 2013; Vohs et al., 2006) and the subsequently presented evidence to bolster trust in money priming effects (Vohs, 2015) are most likely biased by selective reporting and other questionable research practices. Of course, the existence of unreported (and possibly non-significant) studies does not necessarily imply that the effects are not real (Schuler, Vogel, & Wänke, 2017, Appendix C), but it questions the alleged robustness and reliability of money priming effects (Vadillo et al., 2016) and the explanation that the opposing findings in the replication studies were only due to unknown moderators (see Vohs, 2015).

Challenges and Future Questions to the Field

Thus the field faces similar challenges as priming in general:

- Do the different manipulations all work equally effective? Do they reliably activate the 'idea of money'? Do they all prime the same concept?
- *When* does the effect occur? What are limiting factors? Does awareness play a role?
- Which roles play interindividual differences? Do people interpret or perceive the primes differently, thus activating different concepts? Does the prime work equally effective for all people?
- *Why* does the effect occur? What is/are the underlying mechanism(s) for the effect(s)? Can some effects be explained more parsimonious with the same underlying mechanism? Can money primes activate different concepts depending on the environmental setting?

- Does the activation of the 'idea of money' conceptually and empirically differ from effects of other *resource primes* (Nelson & Morrison, 2005), thoughts about one's own money (Pfeffer & DeVoe, 2009) or own expenses (Mogilner & Aaker, 2009)?
- Does money priming really not replicate? Is the hypothesized effect (or some findings) based on Type-I errors?

Some of these questions have been tackled by others during the course of this research, while I tried to tackle some of these questions myself. Yang et al. (2013) found that a small variation of the prime (dirty versus clean money) apparently activates different concepts, which either lead to a tendency for exploitation and greed or to a tendency to obey moral rules. Zedelius et al. (2013) investigated whether awareness alters the effects of money primes and found in line with previous priming research (Newman & Uleman, 1990) that the prime should be perceived without awareness in order to avoid contrast effects. I examined mainly three aspects: When does an effect occur, what exactly do we need to prime and which role play individual differences? Why does an effect occur and which underlying mechanisms can account for the effects? Does money priming replicate and is the skepticism warranted?

Own Research

I started my research with several conceptual replications of former findings and tested whether the effects on helpfulness, just-world-beliefs, immoral behavior, and construal level would replicate in my own research. Moreover, throughout these replications we investigated which alternative underlying mechanisms could account for the findings. Hence, we assessed the motivation to achieve one's goals as a possible mediating mechanism (see Vohs et al., 2008) and developed an alternative resource prime to test whether the feeling of abundant

resources (see Zhou et al., 2009) would produce the reported effects on helpfulness (Roberts & Roberts, 2012). However, our results never replicated the original findings and in the case of helpfulness and just-world-beliefs even pointed in some studies significantly in the opposite direction. Consequently, we also found no evidence in our studies for the proposed underlying mechanisms.

Thus, we started to look more carefully for possible moderators and interindividual differences that could explain our null findings. In our first approach we looked whether attitudes towards money, which have been shown to vary considerably between individuals (Furnham, Wilson, & Telford, 2012; Tang, 1992; Yamauchi & Templer, 1982) might moderate the effects of money priming. People differ in how much meaning and relevance they attach to money in their life and thus more positive views towards money could plausibly activate different concepts than negative views, when being primed with money. Consequently, some first tentative findings indicated that money attitudes might play an important role for money priming (Gąsiorowska & Hełka, 2012; Zaleskiewicz et al., 2013). However, different attitudes towards money, such as whether money was perceived rather as an indicator of success versus an indicator of evil (Tang, 1995) did not moderate the money priming effects on the preference for solitary activities (Vohs et al., 2006), unethical decisions in a hiring scenario (Kouchaki et al., 2013) or general political attitudes (as suggested in: Caruso et al., 2013) in our studies. Moreover, we did once again not find a main effect of money priming in these studies.

A Fresh Look: Does Feeling Privileged or Not Make a Difference?

In our next research project (Schuler & Wänke, 2016, Appendix A), we wanted to investigate two questions that might carry the field forward: Which concepts does money

trigger and hence which underlying mechanisms drive a particular effect? Which role play interindividual differences?

Caruso et al. (2013) primed participants with the concept of money and found that it increases the "endorsement of social systems that legitimize social inequality" (p. 301). More specifically, the authors found that money primes increased ratings on four related dependent variables (System Justification Scale, Belief in a Just World Scale, Social Dominance Orientation Scale, Fair Market Ideology Scale). To explain their findings, they suggested that money serves as a symbol for free-market systems and the establishment that upholds it (Vohs, 2015). Hence, because money is a symbol of the American system of free-market capitalism, money should increase the justification of this system in general and the aforementioned related constructs (Caruso et al., 2013). This was a new and unusual explanation, given that money could serve as a symbol for many things (e.g., shopping, business, success) and therefore the suggested mechanism could predict a wide range of effects. More importantly however, we reasoned that this explanation would actually dissent with former explanations that money evokes a strict focus on the self and the pursuit of personal benefits (see p. 7 for more details), because in line with this reasoning, people should evaluate a system according to how well they succeed in it and how much they benefit from it. We hypothesized that people who feel disadvantaged by their societal system (e.g., because they feel marginalized, unsupported and at the bottom of the hierarchy) should be less likely to support it, whereas people who subjectively benefit from the system (e.g., because they are privileged and succeed in it) should endorse it. Thus we predicted that one's subjective socioeconomic status (subjective SES), which measures a person's subjective standing in society and has shown to play a vital role in people's behaviors and cognitions (Brown-Iannuzzi, Lundberg, Kay, & Payne, 2015; Demakakos, Nazroo, Breeze, & Marmot,

2008; Piff, Kraus, Côté, Cheng, & Keltner, 2010) moderates the effect on system justification.

In our Study 1 we conceptually replicated the experiment on system justification by Caruso et al. (2013), included a manipulation check and assessed participants subjective SES. The results indicated that the prime successfully and reliably activated thoughts of money independent of a person's subjective SES. More importantly, subjective SES moderated the effect on system justification, such that only higher subjective SES led to higher system justification compared to the control condition.

In Study 2 we wanted to replicate this finding with a different measure for system justification that had been used by Caruso and colleagues as well. With the Belief in a Just World Scale, we assessed just-world beliefs, which describe a tendency to perceive the world as fair and consequently as legitimate (Jost & Hunyady, 2005). Additionally, we wanted to test for an alternative underlying mechanism, which could further emphasis the role of interindividual differences for the field. So far, it had not been clear whether money primes only activate the concept of money or whether they remind participants of their own or alternatively other's financial possessions. However some research indicated that this distinction might play a role and different mechanisms would be activated when participants specifically think of others' or of their own money (Gino & Pierce, 2009; Pfeffer & DeVoe, 2009). Regarding our study, thinking of other's money could evoke feelings of injustice, increase social comparisons and a tendency to reject the socioeconomic system, while being reminded of ones own money could lead to greater self-interest and support for the system. We created two priming conditions in which one should remind participants of their own and

the other condition of other people's money.[4] The results bolstered the findings of Study 1. Higher subjective SES lead to higher and lower subjective SES to lower just-world beliefs, both, when subtly reminded of their own and when reminded of other people's money but not (significantly) for participants in the control group. To assess the robustness and the reliability of the effect, we conducted a small-scale meta-analysis and included one previously unreported study in which the manipulation check indicated a weak manipulation. The analysis confirmed our interaction effect.

The findings shed some light on the question whether interindividual differences play a role for money priming. On the one hand, we found no evidence that it makes a difference whether people think of their own or other people's money, which suggests that it is rather important that the concept of money is generally activated. On the other hand, the findings indicate that one's subjective standing in society can alter the effects of money priming. Furthermore, considering participants' subjective SES might also help to explain recent replications that raised doubt about the reliability of the effect (Klein et al., 2014; Rohrer et al., 2015), because the university samples from the original researchers were presumably highly privileged.

Apart from *when* the effect occurs, these findings also give some insight on *why* the priming effect occurs. The effect on system justification might be better explained with an increased focus on the self rather than Caruso's *symbol for free-markets explanation*. Furthermore, because subjective SES did not moderate the effect on the manipulation check but on system justification, it seems that the prime is not perceived differently but rather provokes different behavioral responses. But what implications does this have for the other

[4] As a side note: This manipulation also served to rule out an alternative explanation of our findings on subjective SES. One could argue that participants high in SES simply think of their own money, while participants low in SES think of other people's money, which could then drive the effect.

findings by Caruso et al. (2013)? I believe, not much. The suggested mechanisms of an increased *self-focus* requires that one can express the need to pursue personal interests and benefits in the dependent variable. It is thus unclear whether this mechanism can account for the effect on the Social Dominance Scale (e.g., “Some groups of people are simply inferior to other groups"; Pratto, Sidanius, Stallworth, & Malle, 1994) or the Fair Market Ideology Scale (e.g., "Regulated trade is fair trade"; Jost, Blount, Pfeffer, & Hunyady, 2003). Both scales do not make clear predictions how to answer in one’s own best interest, because they do not specify the role of the participant.[5]

Besides providing direct implications for the largely noted articles by Caruso et al. (2013), Rohrer et al. (2015), and Klein et al. (2014), the findings also have some broader implications for money priming research in general. First, the mental activation of money might not necessarily lead to the same behavioral outcomes and judgments for different people. Second, it appears crucial to carefully look at the theoretical predictions that drive an effect (and maybe examine which other suggested mechanisms could explain an effect as well). Third, unexpectedly, we found that the money primes might not always work equally reliable thus suggesting that large variations in the strength of the very same manipulation are possible.[6]

Maybe because of these implications, maybe because it did „an excellent job of illustrating a ‘hidden’ moderator” (J. T. Crawford, personal communication, May 12, 2016), or maybe because the findings questioned the evidential value of the failed replications (Klein et al.,

[5] It is for example unclear whether participants would perceive themselves as affiliated to the “superior” group of Americans versus immigrants or affiliated to the “inferior” group of low versus high social status and whether fair trade would be beneficial for the self or not.

[6] In fact, in former studies we also found that different primes (images versus descrambling task) caused different effect sizes in the manipulation check, suggesting that the concept of money was not primed equally strong along different manipulations.

2014; Rohrer et al., 2015), this research has been subject to a large scale replication effort itself (Crawford & Fournier, 2017). The replication attempt conceptually replicated the findings on system justification but not on belief in a just world, when using a strict inclusion criterion for participants. More interestingly, it showed the importance to take a closer look at the data of online studies and to exclude participants, who did not work conscientiously at the manipulation task.

Do Money Primes Change Political Views?

The implications of Caruso et al.'s (2013) research seem clear. They showed that money primes influence some of the core beliefs of political right-wing orientation and thereby suggest that – more generally - money primes "change people's political views" (Rohrer et al., 2015, p. e73). Subsequent replications raised doubt about the reliability of the effects (Klein et al., 2014; Rohrer et al., 2015) and their respective suggestion. Moreover, our own research (Schuler & Wänke, 2016, Appendix A) and its implications for the underlying mechanism (*self-focus* instead of *symbol for free-markets* explanation) would open up the possibility that Caruso's findings are a cluster of different effects with different underlying mechanisms, not different behavioral expressions of a general endorsement of free-market systems. Hence, to us it remained an important research question, whether money primes would - beyond changing some underlying right-wing beliefs - indeed change participants' political orientation. In our research, we therefore aimed to thoroughly test whether money primes would affect direct measures of political orientation, which reflect a broader range of motivational and attitudinal aspects than the previously assessed right-wing beliefs (cf. Jost, Glaser, Kruglanski, & Sulloway, 2003).

To test this hypothesis we adopted a multistudy approach of seven studies with different dependent variables of right-wing orientation (Schuler, Ivanov, & Wänke, 2017, Appendix

B). Due to possible variations of the strength (i.e., efficacy to activate the proposed concept) and the activated concept of the different primes, we held the money prime itself constant. We tested two hypotheses and thereby contrasted two possible underlying mechanisms. First, we tested for a main effect of money primes on political right-wing orientation, because money primes were found to increase underlying right-wing beliefs and values (e.g., Caruso et al., 2013; Vohs, 2015). Second, due to our findings on subjective SES, we tested for the interaction effect of money x subjective SES, because the suggested *self-focus* could make people shift their political orientation towards a point where they benefit the most from it. In other words, people who feel socioeconomically privileged should become more right-wing oriented, because right-wing politics, which aim to preserve the social order and which criticize social egalitarianism and redistribution (Esping-Andersen, with Gallie, Hemerijck, & Myles, 2002; Giddens, 1998; Jost, Glaser, et al., 2003; Roemer, 1998), are more in line with their personal interests. On the other hand, people who feel socioeconomically disadvantaged should become more left-wing oriented, because left-wing policies aim to minimize social differences and to provide financial support to disadvantaged groups (Brown-Iannuzzi et al., 2015).

As endorsed in more detail in article 3 (Schuler, Vogel, et al., 2017, Appendix C), we conducted an internal small-scale meta-analysis to avoid unreliable single analysis of studies and to benefit from a maximum of power. The meta-analysis revealed a non-significant main effect in the opposite direction. Considering the confidence intervals (Cumming, 2012), our findings suggest that the effect of money primes on political orientation is either essentially zero or small and in the opposite direction. For our alternative hypothesis that subjective SES moderates the effect, we found tentative, marginally significant evidence. Hence, participants primed with money tended to report stronger (weaker) right-wing orientation when they felt

they had a privileged (disadvantaged) standing in the social hierarchy, compared to participants not reminded of money.

Our goal was to measure general political orientation because we considered the hypothesis that money primes change people's political orientation an important question for basic research and for its relevance in real life. From a theoretical perspective, we were able to identify and test two conflicting hypotheses for money priming. For those interested in a practical perspective of this research, it appears that the menace of money priming for the political life is relatively manageable, due to the small effects that we obtained in relatively controlled settings.

More generally, these findings fit nicely in the pattern of our former research and indicate that subjective SES is an important variable when it comes to priming people with money. However, even though the role of people's subjective standing in society cannot be underestimated in social psychology (Brown-Iannuzzi et al., 2015; Demakakos et al., 2008; Keefer, Goode, & Van Berkel, 2015; Kraus, Côté, & Keltner, 2010; Kraus & Stephens, 2012), I would not predict that it can explain the general unreliability of money priming effects or that it affects most money priming findings. Once again, following the general suggested mechanism that money primes evoke a focus on the self, it should depend on the set of behavioral and possibly even perceptual options (i.e., the dependent variable), whether subjective SES moderates the priming effect or not.

What Can We Conclude From Non-Significant Replications?

The idea that the effects reported by Caruso et al. (2013) might possibly not all reflect the same underlying mechanism (see p. 15 f.) led me to look more closely at the replication results from Rohrer et al. (2015). Intriguingly, Rohrer's replication on fair market ideologies was not significant but clearly pointed in the same direction ($d = 0.14$) as the original finding

(d = 0.70). Thus, - given that a money priming main effect on the business related items of the Fair Market Ideology Scale intuitively seemed not completely preposterous - I began wondering how persuasive the evidence by Rohrer and colleagues was.

I myself had conducted series of conceptual replications, many of which turned out to be non-significant. For example in my own findings, only one out of four studies was statistically significant (see Schuler, Ivanov, et al., 2017, Appendix B), a pattern that would probably lead most researchers to favor a Type-I error explanation (for the significant finding). However, a meta-analytic effect with a p-value of .07 would probably guide most researchers to more careful interpretations. Hence, more generally I wondered: How strong is the evidence of a replication that points in the same direction as the original effect? When facing such common patterns of significant and non-significant findings, should we look for moderators or should we expect such mixed results? Do sets of non-significant replications imply that an effect does not exist?

These initial thoughts on the replicability of this money priming effect inspired a broader methodological report on non-significant replications, their interpretation and their evidential value, that burst the limits of mere money priming (Schuler, Vogel, et al., 2017, Appendix C). Our goal was to specifically address non-statisticians in social psychology and other fields to think about non-significant replications in a non-technical and easily accessible way. More specifically, we wanted (a) to raise awareness for a shortsighted interpretation of non-significant replications among researchers and readers of the psychological literature, (b) illustrate the detrimental consequences from exaggerated conclusions in replications to discarded research projects, and (c) contrast these common interpretations by showing that non-significant replications can also increase the evidence for an effect.

In a first step, we outlined unwarranted beliefs about non-significant replications. One states that “non-significance does provide evidence for a null effect” (confirming the null

hypothesis) even though in fact it only shows no significant evidence in favor of an effect (rejecting the alternative hypothesis). Another belief states „non-significance questions the existence of an effect" although this is not necessarily true when the effect is considered in the larger context of other data. We then exemplified the prevalence of such beliefs with claims in replication reports and original findings and with own data from German Psychology students. In our study we presented participants with scenarios of mixed findings (significant and non-significant results) and found that they disregard the possibility of a smaller effect and rather conclude that an effect is not real (i.e. null). Such a single-study-mindset in which participants possibly count significant against non-significant results is flawed. If we test ordinal hypotheses, the exact underlying effect size does not really matter, thus a non-significant replication indicating a smaller effect would still be in line with the hypothesis. More importantly, such a non-significant replication does also neither provide evidence for a null-effect nor does it necessarily question the effect. On the contrary, in a meta-analytic setting it could even increase the overall evidence for true effect.

To illustrate this point, we showed in a second step how non-significant replications can decrease but also increase the evidence for an effect and therefore do not necessarily provide evidence against but also in favor of an effect. Because similar beliefs have been voiced about unreported, "file-drawer" studies (Rohrer et al., 2015), we repeated such an analysis for unreported "file-drawer" data. To emphasize the relevance of our article for social psychology researchers and replicators we used reported replication data (and unreported "file-drawer" studies), rather than simulations (see Braver, Thoemmes, & Rosenthal, 2014).

Even though, the message of this report was outlined so far, we wanted to give a complete introduction on how to deal with a set of mixed findings. Thus, in a third step we discussed different approaches to evaluate – based on the available data and depending on whether one trusts the original findings – whether an effect exists or not. Generally, we suggest to

consider all available data (either "internal replications" within one research project or original and "external" replication studies) in a meta-analytic approach that integrates the different effects. Because both original studies and replications studies can be biased and replications can represent different underlying effects, we further suggest to use heterogeneity analysis to detect for such differences.

The logic of a cumulative science and the obstacles of interpreting non-significant findings are currently discussed in blogs and certain social media groups (e.g., Schimmack, 2016, November 23; Simonsohn, 2016, December 17). However, we experienced that the rationale that non-significance does not necessarily speak against a hypothesized effect is not yet common knowledge (at least within social psychology), and that it still leaves many psychologists on conferences somewhat surprised. Hence, this article was not intended as another paper on replication or to propose new ways to evaluate the success of a replication, but to encourage social psychologists and others to think of Psychology in general and their own findings in particular more as a cumulative science, rather than focusing on single *p*-values. This would facilitate the view that non-significance is an unreliable indicator for the existence or non-existence of an effect and that non-reported studies are a necessary but not a sufficient condition to demonstrate the non-existence of a previously suggested effect.

Nowadays researchers might tend to sort replicability contributions in those from crisis-believers and those from crisis-deniers, who (at one end of the continuum) disagree that any problem of replicability or bad research practices exists. When reading our article, one might be inclined to quickly put it in the denier's drawer. But, even though we suggest that non-significance should not be overrated, this does not mean we want to take that side in this debate. On the contrary, to my point of view, questionable research practices such as reporting bias, *p*-hacking, file-drawer biases et cetera are heavily detrimental for our science and we need better theorizing (Fiedler, 2014, 2017; Platt, 1964) and better standards for

analyses (Vazire, 2016) to improve our science. However, it seems too easy and simply unwarranted to question the validity of an effect based on a single or a set of non-significant replications, - even with a ceteris paribus assumption that ignores possible moderator explanations for the different effects -, without considering the remaining original data or the overall evidence of the effect.

But what are the implications of this contribution for money-priming? Does money priming really not replicate or is it all a question of subtle unknown moderators (Cesario & Jonas, 2014)? Money priming effects did not reliably replicate in some studies and this could be due to subtle moderators or the validity of the effect. Thus, skepticism about the effects is certainly warranted. It is however unwarranted to sweepingly conclude that money priming is not real at all because some researchers could not replicate the effect, others engaged in *p*-hacking or because some original studies were not included in the final report. As shown in our research (Schuler & Wänke, 2016, Appendix A), even a set of 23 "failed" replications (Klein et al., 2014) can be relatively easy explained when the original study was based on a highly selective sample (see Caruso et al., 2013). Hence, we cannot ultimately decide whether these money priming effects exist and the debates about the implications of so called "failed" replications will continue in both directions (Fabrigar & Wegener, 2016; Gilbert, King, Pettigrew, & Wilson, 2016; Maxwell, Lau, & Howard, 2015; Open Science Collaboration, 2015; Pashler & Harris, 2012; Pashler & Wagenmakers, 2012; Simmons, Nelson, & Simonsohn, 2011; Simonsohn, 2015; Stroebe & Strack, 2014).

Does Money Evoke a Focus on the Self?

But how could we investigate the validity of a field (i.e., whether the proposed effects are real or not) in a way that would leave less room for speculation about unknown subtle moderating factors (Cesario, 2014), the contextual sensitivity of an experiment (Van Bavel,

Mende-Siedlecki, Brady, & Reinero, 2016), incapable replicators (Luttrell, Petty, & Xu, in press; Schwarz & Strack, 2014), and natural statistical variation of the effect (Schuler, Vogel, et al., 2017, Appendix C; Stanley & Spence, 2014)? Some suggest to rely strictly on exact replications (Simons, 2014) that may be even conducted only by expert researchers (Bench, Rivera, Schlegel, Hicks, & Lench, 2017; Kahneman, 2012) and others propose technically advanced "data-inspection systems" that allow to correct for some biases and to detect others (Schimmack, 2014; Simonsohn, Nelson, & Simmons, 2014). Other promising approaches are to preregister research (Unkelbach, 2016) in order to avoid possible biases and to increase the credibility of the findings in a field or to improve and specify the theoretical concepts to create powerful and testable theoretical frameworks for the respective field (Fiedler, 2014, 2017).

Our idea to carry the field forward was to (a) look closer at the theoretical mechanisms and assess the suggested underlying mechanism (b) with a highly reliable measure in (c) a preregistered study (Genschow, Schuler, Cracco, Brass, & Wänke, 2016; Appendix D). The way from a manipulation (e.g. the money prime) to the behavioral response (e.g. reduced donations to a charitable project) is long and many factors could potentially deteriorate (e.g., failed manipulation, reliability of dependent variable) or even cause (confounding variables) the effect. Thus, one step closer to a clear picture of the effects of money priming might be to assess one of the suggested mediating mechanisms rather than a hypothesized dependent variable. Furthermore, we intended to use a highly reliable measure, because a common problem in psychology is the use of unreliable (e.g., single-item) measures to assess the dependent variable (Epstein, 1980). Especially in priming research, the dependent variable often consists of one single behavioral outcome, such as the chosen distance to another person or the amount of pencils that were picked up by the participant. Not surprisingly, the questionable reliability of these measures is also highly problematic for the replicability of an

experiment (Stanley & Spence, 2014). We thus aimed to use a highly valid and reliable measure that reduces measurement error. To increase the credibility of our findings among money priming *skeptics* and *believers*, we aimed for a peer-reviewed, preregistered publication that allows to publish the theoretically relevant results regardless of the subsequent results.

As outlined in more detail above, a key assumption of money priming that has been described in various articles is that money primes trigger an increased focus on the self and a decreased focus on others (Gasiorowska et al., 2012; Reutner & Wänke, 2013; Vohs et al., 2006, 2008). This "self-focus" on one's own benefits, goals and needs is the suggested mediating mechanism for a great variety of findings (see above; Boucher & Kofos, 2012; Chatterjee, Rose, & Sinha, 2013; Yang et al., 2013). We wanted to test this assumption that I also used to explain our own findings (Schuler, Ivanov, et al., 2017, Appendix B; Schuler & Wänke, 2016, Appendix A) with a reliable task. Hence, the inhibition of imitation measure (Brass, Bekkering, Wohlschläger, & Prinz, 2000), which we borrowed from cognitive psychology and which shows high reliability due to its repeated trials seemed like a highly suitable measure to assess self-focus. In the task, participants have to respond to a visual stimulus (a letter) by raising either their index or their middle finger, while a hand on the screen performs either a congruent or incongruent movement. In the incongruent condition, the observed movement interferes with the instructed movement and therefore has to be inhibited. Research has shown, that the greater the self-focus of a person the less the person mimics others and the smaller the interference with one's own reaction (see Genschow et al., 2016; Appendix D). In other words, while the common effect in such a task is that participants react faster in congruent trials than in incongruent trials (interference effect), the effect is reduced the stronger participants focus on the self.

We conducted a pilot study with a 2 (incongruent vs. congruent) x 2 (control vs. money) within-subject design in which the displayed hand that had to be ignored was grounded on a grey surface (control condition) or a surface full of money bills (money condition). Our results supported the self-focus hypothesis and showed a significant interaction effect on the latencies indicating smaller interference effects in the money condition. However, in our preregistered study, we want to change one aspect in order to rule out an alternative explanation. Because the money bill surface might have grasped participants' attention, we plan to use a similarly "distracting" surface as in the money priming condition.[7]

The lack of theoretical rigor possibly led to the field's current state in which it is unclear which mechanisms and which effects are real. Most of the suggested mechanisms on money priming appear more as post-hoc explanations for obtained findings rather than theoretically derived predictions. Moreover, many of the explanations resemble surrogates (Gigerenzer, 1998), this is "one-word explanations" (p. 2) such as *self-sufficiency* and "rediscriptions" (p. 3) such as *priming makes the concept of money salient* rather than specified theories with precise predictions. Therefore, we considered it more important to test one of the suggested theoretical mechanisms even though the proposed theory makes only few testable predictions (see Vohs et al., 2008) rather than to search for new priming effects. That priming participants with money increases their self-focus is such a testable prediction, albeit it is only a subfacet of the proposed underlying process that comes along with the activation of the psychological concept of money.

Of course, our paradigm does not completely rule out all alternative possible mechanisms. As one example, money priming is supposed to lead to increased performance (Boucher &

[7] Even though this is unlikely, because if the money-surface distracted people from the letter (i.e., the visual stimulus), we would have found greater latencies in the money condition in congruent and incongruent trials (i.e., a main effect). However, this was not the case.

Kofos, 2012). However, in this case we would expect a main effect of the money condition indicating smaller latencies and better performance in incongruent and congruent trials. But, one could also think of other possible mechanisms. For example (not as a theoretically grounded but a fairly hypothetical possibility), the primes could evoke feelings of loss or thoughts about not having as much money, which in turn would lead to rumination and less imitation (Muller, 2017). This would then produce similar behavioral reactions on the task as the hypothesized interaction effect. Be that as it may, the characteristics of this paradigm as being unobtrusive, highly reliable, and less susceptible to contextual influences will help to narrow down possible conclusions that can be drawn from this study. Hence, this preregistered study might be a first step to decrease the range of possibilities and open questions regarding the psychological mechanisms of money priming.

General Discussion

Caruso and colleagues' research (2013) left many open questions and skepticism about their findings, some of which could be tackled in this research. It appears that the findings from Caruso and colleagues are unreliable and difficult to replicate (Klein et al., 2014; Rohrer et al., 2015; Vadillo et al., 2016). First of all, this might be due to the possibility that Caruso's findings are a cluster of different effects with different underlying mechanisms that are only ostensibly related. This alone however would not explain a lack of replicability. But if the findings - or one part of them - are a mix of Type-I errors and questionable research practices that altogether give the impression of a reliable effect, this would be one suggested possibility (Rohrer et al., 2015). Another possibility is that the use of selective samples (see also Vohs, 2015) and the existence of moderators that can set off or even reverse an effect are responsible for the variation of one or more of the reported effects. Lastly, an unreliable activation of the concept of money (i.e., a failure to consistently prime money) could

potentially explain some of the non-significant (and especially some of the online-) replications.

Our research sheds some light on these possibilities. We found in our research that two of these effects (increased system justification & just-world-beliefs) are moderated by subjective SES (Schuler & Wänke, 2016, Appendix A). Participants with a higher subjective SES tend to justify the existing socioeconomic system in the United States more strongly and to believe more in the justness of its social outcomes, when being reminded of money. Participants with a lower subjective SES tend to reject the socioeconomic system and to question the justness of its social outcomes more strongly, when being primed with money. We also found first signs that the money descrambling task works unreliable in online studies and that corresponds with some of our unpublished findings, where we found considerable differences of the amount of money words created in the manipulation check between different priming manipulations (images versus descrambling task). Thus, the role of subjective SES, selective sampling by Caruso and the unreliable activation of the concept might explain some of the unsuccessful replications.

As an answer to the question whether money primes can change political orientation in general – as implied by Eugene Caruso -, we investigated in a multi-study approach and with direct measures of political orientation the effects of money primes (Schuler, Ivanov, et al., 2017, Appendix B). We found no main effect of money primes and a tendency for an interaction effect with subjective SES. In other words, it appears that among our German participants, money priming does not generally affect political views but only as a function of subjective SES. Thus, our data - at least partly - limits and specifies the implications given by the research of Caruso and colleagues.

Furthermore, we showed that the mentioned “file-drawer” studies by Caruso et al. (Rohrer et al., 2015) should cause no skepticism about the effect itself (Schuler, Vogel, et al., 2017,

Appendix C). The studies increased the overall evidence for a smaller but true effect. Once again, it does not follow that the effect on fair-market ideologies is real and that a meta-analysis across all existing findings from Caruso and others would lead to a supportive result for the effect; but it questions the conclusions that the effect on fair-market ideologies (assuming separate independent effects, not one common effect on system-justifying ideologies) can be explained away as being Type-I errors.

More generally, this research has three important implications for the field. First, it seems that the suggested self-focus mechanism for money primes has some validity, because it can explain a comparably complex interaction pattern across several studies (Schuler, Ivanov, et al., 2017, Appendix B; Schuler & Wänke, 2016, Appendix A) and some preliminary evidence bolsters the assumption of a self-focus (Genschow et al., 2016, Appendix D, Pilot Study). Second, we showed that money primes can be moderated by subjective SES but we found no evidence for a subcategory of money primes (i.e., "my" versus "other's" money). Third, the primes may not reliably activate thoughts of money. Thus, this research suggests some explanatory factors, why money priming seems so hard to replicate in general.

Speaking about replications and research in experimental and social psychology in general, our contribution on non-significant findings (Schuler, Vogel, et al., 2017, Appendix C) hopefully provides new insights and maybe even a new perspective for researchers and students in social psychology and other fields. In times of a crisis of confidence in psychology and especially in social psychology, many new replications providing non-significant results are emerging on a monthly basis. Furthermore, journals increasingly require higher power and sets of conceptual replications instead of single-study papers (Vazire, 2016). Thus, to my view it is more than ever of vital importance to acquire a coherent and proper understanding of non-significance in general (cf. Cohen, 1994) and of the meaning and the implications of non-significant replications in particular. Because we felt

that some of these old statistical truisms regarding non-significance are forgotten or maybe not fully ingrained among all researchers anymore, our methodological contribution was intended to fill this gap and raise awareness for an apparently often shortsighted understanding of the evidential value of single replications. I hope it will ultimately lead more researchers in the field to think of their own studies as a set of cumulative evidence that leads to the inclusion of non-significant studies and consequently less file-drawer biases and less biased effect sizes for replications.

During the course of this research it was still not clear, whether and how it would be feasible to include non-significant studies into a publishable manuscript. We tried, discussed it with the editor, reviewers, and colleagues and ultimately found a way to mention and describe our non-significant, "failed" study in the manuscript along with the significant findings without diluting from the story of the manuscript (Schuler & Wänke, 2016, Appendix A). Hence, maybe the two examples we provided on how to include non-significant findings in a manuscript (Schuler, Ivanov, et al., 2017, Appendix B; Schuler & Wänke, 2016, Appendix A) will help our plea to think, test, and report more meta-analytically.

Closing Remarks

One might argue that instead of discovering new money effects or testing certain proposed underlying mechanisms, I could have focused on alternative explanations for the effects and test them. Indeed, one could imagine other possible mechanisms, such that money primes are simple goal primes or reminders of hidden incentives. Furthermore, money primes could be a certain type of power prime, resource prime or business prime rather than evoking an independent priming concept. Maybe future research will lead to some new insights in this regard. Because it would have been difficult to demonstrate such alternative accounts, when

one cannot replicate any of the former findings properly and because there was an increasing skepticism about the findings of money priming in general, I focused in my research on the assumptions made in the literature and on moderating factors. Proposing even more alternative mechanisms would have, to my point of view, not help to advance the field.

The omnipresent use and propagation of meta-analyses is evident in this work. I did not further elaborate on the use and logic of cumulative and small-scale meta-analysis in this synopsis because meta-analysis is only a promising tool to solve statistical problems and to arrive at better conclusions. As a social psychologist, the topic of this research was money, self-focus, politics and the implications of non-significant replications (which appear to occur quite often in money priming), not a statistical method. However, some final words might be appropriate at this point. Small-scale meta-analysis describes the application of a meta-analysis with very few studies (from two up to 20) while a cumulative meta-analysis describes the approach to include a new study (i.e., a conceptual replication) into the meta-analysis of former findings and then see how this new study changes the implications of the data. To learn more about the statistical approach, random-effects analysis, and heterogeneity, the interested reader might read the respective introduction passages on meta-analysis in the appendices A to C or Borenstein, Hedges, Higgins, and Rothstein (2009) as a non-technical introduction to the topic.

Recent publications that received considerable attention, propagate the use of meta-analysis for small sets of studies (Fabrigar & Wegener, 2016; Stanley & Spence, 2014) or in a cumulative approach (Braver et al., 2014; Cumming, 2014) to decrease the confidence interval and to obtain more reliable results. However, some researchers have criticized the use of small-scale meta-analysis, because they could lead to Type-I error inflation and "meta-hacking" (Sakaluk, 2016; Ueno, Fastrich, & Murayama, 2016), be biased due to the inclusion of biased original studies (Sakaluk, 2016) and because it does not substitute for

preregistration (Van Elk et al., 2015). In light of many replications, a great plethora of findings in the field and strong variations in the obtained effect sizes, a meta-analytic approach seemed most promising, even though other approaches to investigate the reliability of an effect or to replicate an original finding also have their benefits (Open Science Collaboration, 2015; Simonsohn, 2015; Verhagen & Wagenmakers, 2014).

Conclusions

While the last eleven years have revealed a confusing wealth of money priming findings, few researchers have focused on the mechanisms and moderating factors that produce these effects. The current research aimed to deal with these issues and provides a somewhat brighter outlook on the psychological processes behind money priming. Furthermore, our theoretical report also provides some implications for other fields on dealing with non-significant findings. Therein our research might offer a new perspective for social psychologists on their own findings and possibly stimulates more sustainable research in money priming that ultimately builds up to a solid theoretical framework that will revive the field.

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Statement of Originality

Eidesstattliche Versicherung gemäß § 9 Absatz 1 Buchstabe e) der Promotionsordnung der Universität Mannheim zur Erlangung des Doktorgrades der Sozialwissenschaften:

1. Bei der eingereichten Dissertation mit dem Titel „Money, Self-focus, and Politics - A Critical Examination of Money Priming, Its Mechanisms, and Its Effects on Political Attitudes" handelt es sich um mein eigenständig erstelltes eigenes Werk.
2. Ich habe nur die angegebenen Quellen und Hilfsmittel benutzt und mich keiner unzulässigen Hilfe Dritter bedient. Insbesondere habe ich wörtliche Zitate aus anderen Werken als solche kenntlich gemacht.
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Johannes Schuler, Mannheim den

Co-Author Statements and Appendices

First statements and then appendices appear in the following order:

(A) Schuler, J., & Wänke, M. (2016). A Fresh Look on Money Priming: Feeling Privileged or Not Makes a Difference. *Social Psychological and Personality Science, 7*, 366-373. doi:10.1177/1948550616628608

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(C) Schuler, J., Vogel, T., & Wänke, M. No Replication, no Effect? What Can We Conclude From Non-Significant Replications?. *Manuscript submitted at Social and Personality Psychology Compass.*

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I hereby confirm that Johannes Schuler is the main author of the following article. He has primarily conceived and written the manuscript, and is the main source of ideas expressed therein. He also prepared, conducted and analyzed the studies described in the article and implemented reviewer comments for the revisions.

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I hereby confirm that Johannes Schuler is the second author of the following article. Together with Oliver Genschow he is responsible for the initial idea for this research. He has primarily written the theoretical part of this paper and, together with Oliver Genschow and Emiel Cracco, worked on the implementation of the reviewer comments for the revision.

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A fresh look on money priming:
feeling privileged or not makes a difference

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Abstract

Two studies demonstrated that subjective socioeconomic status (SES) moderates the effects of reminders of money on the endorsement of the socioeconomic system (see Caruso et al., 2013). Whether reminders of money increased or decreased system justification (Study 1) and the belief in a just world (Study 2) depended on participants' subjectively experienced standing in the social hierarchy. These findings were backed up by a small-scale meta-analysis across our entire data ($N = 365$). Hence, we also included a third study into the meta-analysis, in which the manipulation check indicated that the mental activation of money was comparably weak. This research offers new insights into the psychological mechanisms of money primes and reveals that interindividual differences, such as whether one feels privileged or not, can moderate the effects of money primes.

Keywords: money, priming, socioeconomic status, meta-analysis, reproducibility

Introduction

Money plays an important role in our lives. Researchers have shown that, to some extent, personal wealth affects one's happiness (Diener & Seligman, 2004; Dunn, Aknin, & Norton, 2008), health and longevity (Fiscella & Franks, 1997; Mackenbach et al., 2005), and the ability to experience positive emotions (Quoidbach, Dunn, Petrides, & Mikolajczak, 2010). Moreover, besides *having* money, research in the last few years has revealed that even thinking about money can have tremendous effects on people's behavior and attitudes (Vohs, Mead, & Goode, 2006). Overall it seems that people who are subtly reminded of money "behave as if they can do just fine without others" (Lasaleta, Sedikides, & Vohs, 2014, p. 714). They tend to care less about others and focus more on their own personal needs and goals (Vohs et al., 2006; Vohs, Mead, & Goode, 2008). Findings that being reminded of money made people more accepting of social inequality (Caruso, Vohs, Baxter, & Waytz, 2013) would fit this general tendency. Caruso and colleagues (2013) claim that money serves as a symbol (i.e., as a reminder) of free-market capitalism and should lead people to endorse such free-market systems and more generally the existing free-market system in the United States. Accordingly, these authors found higher scores on the System Justification Scale (Kay & Jost, 2003) and the Belief in a Just World Scale (Rubin & Peplau, 1975) among US-participants who had been reminded of money.

In the present paper we take another look at the theoretical assumptions that stimulated this research and derive more differentiated hypotheses. In this respect it may be interesting to note that other studies could not replicate the original findings (Klein et al., 2014; Rohrer, Pashler, & Harris, 2015). Indeed, according to our reasoning, the effect may strongly vary depending on participants.

The present research

An established finding of money priming is that reminders of money lead to a focus on the self and one's personal advantages (see: Vohs, 2015). Money reminded participants exert more effort on their current tasks (e.g., Boucher & Kofos, 2012; Mogilner, 2010) and are more concerned with their own compared to others' benefits (Gąsiorowska & Hełka, 2012; Reutner & Wänke, 2013). They are also more likely to behave unethically in order to achieve money or other goals (Gino & Mogilner, 2014; Kouchaki, Smith-Crowe, Brief, & Sousa, 2013). In summary, while neglecting others' interests, it appears that money evokes a bias for personal concerns and self-interest (Gino & Pierce, 2009; Kouchaki et al., 2013; Vohs et al., 2006, 2008).

Thus, when money motivates to pursue personal benefits, one should be more likely to evaluate a system according to the extent it serves one's personal needs. In other words, those who think they profit from the existing socioeconomic system should be more likely to approve of it when reminded of money. Accordingly, those who feel less advantaged by the existing system should be less likely to support it when reminded of money. Socioeconomically unprivileged people face tougher challenges (Dohrenwend, 1973; Johnson & Krueger, 2006) and have fewer coping resources (McLeod & Kessler, 1990) than their more advantaged counterparts. More importantly, people with a better education, more money and better jobs benefit from free-market systems, whereas people with less education and money suffer from them compared to more egalitarian systems (Esping-Andersen, with Gallie, Hemerijck, & Myles, 2002).

We would therefore predict that whether reminders of money lead to higher endorsement of the US socioeconomic system depends on who is asked. We expect that an individual's perceived standing in the social hierarchy (i.e., subjective socioeconomic status, *subjective SES*) moderates the effect of money on the justification of the prevailing

socioeconomic system. Because reminders of money cause people to evaluate a system according to how well they are doing in it, individuals who see themselves as socioeconomically disadvantaged should be less inclined to justify the existing system. On the contrary, people who would view themselves more as the beneficiaries of the socioeconomic system should endorse the system after being reminded of money.

Even though indicators of an individual's objective socioeconomic status form the basis of one's subjective SES (Demakakos, Nazroo, Breeze, & Marmot, 2008), they capture different conceptual dimensions (Brown-Iannuzzi, Lundberg, Kay, & Payne, 2015; Demakakos et al., 2008; Piff, Kraus, Côté, Cheng, & Keltner, 2010). We emphasize that it is the subjective experience of one's standing in the social hierarchy that influences feelings of being privileged or disadvantaged.

We conceptually replicated two experiments by Caruso and colleagues (2013), and assessed system justification as a function of money priming. Critically, different from the original studies, we used a broader sample in terms of its socioeconomic background and assessed participants' subjective SES.

Study 1

Method

Design and participants. We measured subjective SES for half of the participants before the manipulation and for the other half after the dependent variable. Thereby we controlled for possible influences of the money prime on participant's subjective SES reported after the dependent variable as well as for a potential priming of money due to the assessment of subjective SES before the manipulation. This resulted in a 2 (Prime: money vs. control) x 2 (SES-order: before vs. after) design with subjective SES measured on a 10-point scale.

Participants were recruited through the online platform CrowdFlower. We predetermined our sample size with 130 (plus 20 participants buffer for possible dropout due to online recruitment) to arrive at a power of $1-\beta > .80$ assuming a small to medium interaction effect ($f^2 = 0.0625$; alpha = .05; 2-tailed). From 150 participants, four left no data and data from another 22 participants were excluded because they did not follow the instructions to create four-word phrases in the manipulation. The remaining 124 participants (62 males, 1 other) were on average 38 years old (*SD* = 12.92), 60.5% had a household income of less than $50,000 per year, and 58.5% did not graduate from college. We exclusively recruited US-American participants in order to ensure the effectiveness of the semantic prime and due to the "America-centric version of the System Justification Scale" (Caruso et al., 2013, p. 304).

Materials and procedure. Participants accessed the study online and proceeded to a word-descrambling task, which consisted of forming a grammatically correct four-word phrase out of five scrambled words. The money condition contained 15 money-related phrases (e.g., "one hundred dollar bill") and 15 control phrases (see Appendix A; Vohs et al., 2006). Next, to measure the cognitive activation of money, participants worked on a word stem completion task in which they had to complete 15 word stems, seven of which could be completed as money-related or neutral words (e.g., "ri-" as rich, rise or ride; see Appendix C; Vohs et al., 2006). Then participants completed the eight-item System Justification Scale (e.g., "American society needs to be radically restructured"; reverse coded; $\alpha = .78$; Kay & Jost, 2003), as adapted by Caruso et al. (2013; 1 = *strongly disagree*; 7 = *strongly agree*). Finally, participants reported demographic variables including measures for household income (Piff et al., 2010), political ideology and religiosity.

To assess a person's subjective SES, participants completed an online version of the MacArthur Scale of subjective SES (Piff et al., 2010). Participants were instructed to imagine an image of a 10-rung ladder as representing where people stand in the United States, whereby the top rung represents the highest standing in the social hierarchy. Then individuals indicated on a 10-point scale where they see themselves on this ladder relative to others (M = 5.66, SD = 1.98; $r_{\text{(household income)}}$ = .29).

Results and Discussion

A t-test revealed that money-reminded participants created significantly more money words (n = 58, M = 1.31, SD = 0.96) than when not reminded of money (n= 66, M = 0.77, SD = 0.78), $t(122) = 3.44$, $p < .001$, d = 0.62, 95% confidence interval (CI) = [0.26, 0.98]. This effect was similar to previous studies using this method (Vohs et al., 2006) and not moderated by subjective SES, b = 0.02, $t(120) = 0.44$, p = .66, 95% CI = [-0.06, 0.10].

To test our moderation hypothesis, we regressed system justification on the prime (-1 = control, 1 = money; mean-centered[1]), subjective SES (1 – 10; mean-centered), SES-order (-1 = before manipulation, 1 = after dependent variable; mean-centered) and all interaction terms. For participants with an average value in subjective SES, the money prime (M = 4.03, SD = 1.11) did not lead to stronger system justification than in the control condition (M = 3.94, SD = 0.88), b = 0.05, $t(116) = 0.56$, p = .58, 95% CI = [-0.12, 0.22]. Subjective SES significantly affected participants' system justification, b = 0.14, $t(116) = 3.26$, p = .002, 95% CI = [0.06, 0.23] with people higher in subjective SES reporting higher system justification. More importantly, the expected interaction of money and subjective SES significantly predicted system justification, b = 0.11, $t(116) = 2.46$, p = .02, 95% CI = [0.02, 0.19], f^2 =

[1] As suggested by Hayes (2013), contrast variables throughout all studies were mean centered to provide estimates of the weighted average effects at the sample mean level of all other variables.

.05, [0, 0.12].[2] [3] This interaction was not affected by SES-order. Neither the three-way interaction, b = -0.03, $t(116) = 0.68$, p = .50, 95% CI = [-0.12, 0.06], nor the two-way interactions SES-order x Prime, b = 0.14, $t(116) = 1.64$, p = .11, 95% CI = [-0.03, 0.31], SES-order x Subjective SES, b = 0.02, $t(116) = 0.51$, p = .61, 95% CI = [-0.06, 0.11], or the effect of SES-order were significant, b = 0.01, $t(116) = 0.09$, p = .93, 95% CI = [-0.16, 0.18]. Collapsing over SES-order, simple slope analysis (Hayes, 2013) revealed a small but not significant effect of subjective SES on system justification in the control condition, b = 0.06, $t(120) = 0.90$, p = .37, 95% CI = [-0.07, 0.19]. However, as shown in Figure 1, higher subjective SES led to an increase in system justification in the money condition, b = 0.25, $t(120) = 4.52$, $p < .001$, 95% CI = [0.14, 0.36]. In summary, whether reminders of money increased system justification depended on where people see themselves in the system. Whether subjective SES was assessed before the manipulation or after the dependent variable did not affect these results significantly. Moreover, the mental activation of money due to the money prime did not vary significantly across different levels of subjective SES.

[2] Controlling for religiosity (only in Study 1), age, gender, political ideology, and household income (see Caruso et al., 2013) did not significantly alter the results in both studies. The reported confidence intervals for *f*-square are 95% one-sided, because *f*-square effect sizes cannot be negative.

[3] When applying a more liberal dropout criterion that comprised outliers who interrupted the study (n = 7; >=19.1 min, i.e. upper quartile + 1.5 interquartile range) and non-native speakers (6), the interaction remained significant, b = 0.11, $t(103) = 2.29$, p = .02, 95% CI = [0.01, 0.20].

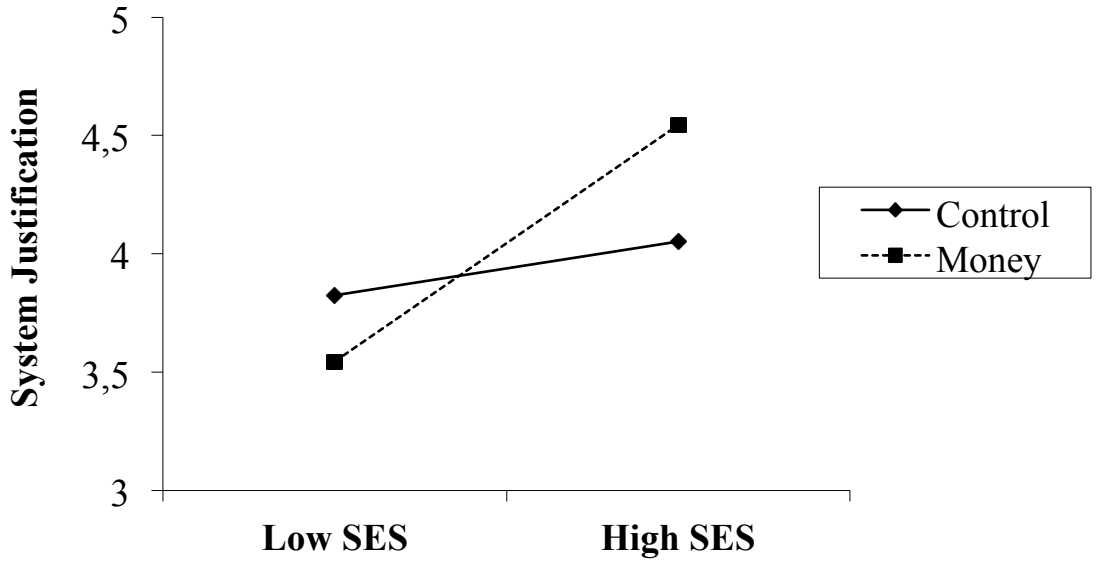

Figure 1. Estimated System Justification scores by condition (control vs. money) for lower (1 *SD* below the mean) and higher (1 *SD* above the mean) subjective socioeconomic status (SES; 1 - 10) collapsed over SES-order (before manipulation vs. after dependent variable).

Study 2

We conducted another study to broaden our findings by using a different measure (Belief in a Just World Scale; Rubin & Peplau, 1975), which had also been shown to be sensitive to money priming (Caruso et al., 2013; Study 2). The belief in a just world (BJW) describes a tendency to rationalize the status quo by blaming victims for their fate (Jost, Pelham, Sheldon, & Ni Sullivan, 2003). As such BJW is thought to be a system-justifying ideology to legitimize the current system (e.g., Jost & Hunyady, 2005) and the Belief in a Just World Scale (Rubin & Peplau, 1975) is used as a measure for general system justification (e.g., Jost & Burgess, 2000; Oldmeadow & Fiske, 2007).

Again we predicted that being reminded of money should increase approval and belief in the system for those who are doing well in the system, but less so for those who are less privileged. For exploratory reasons, we also tested whether thoughts of one's own versus other people's money would lead to different effects. On the one hand, one might assume that

thinking about one's own money might possibly be more effective in inducing self-interest than thinking about other's money. This would suggest that participants' subjective SES affects their endorsement of the US-American system more, if they were reminded of their own rather than others' money. On the other hand, thoughts about other people's money might evoke greater subjective comparisons with others and thereby induce similar or greater effects with regard to just-world beliefs.

Method

Participants and design. In addition to a control group, two money prime groups were run in order to realize the different perspectives (my-money vs. their-money). Similar to Study 1, we predetermined our sample size with 150 (including 20 participants buffer) to arrive at a Power of $1\text{-}\beta > .80$. From the initial CrowdFlower sample of 150 US-American participants, nine left no data and data from 23 participants were excluded because they did not produce four-word phrases in the descrambling task. The remaining 118 participants (40 males, 2 other) were on average 40 years old (SD = 14.03), 51% had a household income of less than $50.000 per year, and 65% did not graduate from college.

Materials and procedure. Analogous to Study 1, participants accessed the study online and completed a modified version of the same descrambling task. Different from Study 1, we varied all personal pronouns in the 15 money-related phrases in order to evoke thoughts of one's own money in the my-money condition (e.g., "I received a raise") and to evoke thoughts of others' money in the their-money condition (e.g., "She received a raise"; see Appendix B). In other words, whereas the money-related phrases in Study 1 contained mixed or no personal pronouns (e.g., "I", "we", "their", "he"), money-related phrases in the my-money condition contained only self-related (e.g., "I", "our", "my") and money-related

phrases in the their-money condition contained only other-related (e.g., “he”, “her”, “their”) personal pronouns. Afterwards participants completed the 20-item Belief in a Just World Scale (e.g., "It is rare for an innocent to be wrongly sent to jail"; α = .75; Rubin & Peplau, 1975) as an adapted version by Caruso et al. (2013; 1 = *strongly disagree*; 7 = *strongly agree*). Finally, participants reported their subjective SES (M = 5.73, SD = 2.06; $r_{\text{(household income)}}$ = .48) and demographic variables similar to Study 1.

Results and Discussion

We used two orthogonal-contrasts in a regression analysis. The first contrasted both “my” and “their” money groups versus the control group (0.5, 0.5, -1; mean-centered) to test for a general money effect. The second contrasted “my” versus “their” money (-1, 1, control = 0; mean-centered) to test for differences between being reminded of one’s own versus others’ money. We further included the mean-centered subjective SES and both interaction terms.

Both contrasts did not reveal significant differences between conditions at the mean level of subjective SES. Participants in the money conditions (M = 3.99, SD = 0.68) did not report stronger just-world beliefs than in the control condition (M = 4.05, SD = 0.66), b = 0.02, $t(112) = 0.28$, p = .78, 95% CI = [-0.14, 0.19]. Furthermore, whether participants were primed with their own money (my-money-condition; M = 3.92, SD = 0.70) compared to other people’s money (their-money-condition; M = 4.07, SD = 0.67) did not alter the results between groups, b = -0.04, $t(112) = 0.55$, p = .58, 95% CI = [-0.19, 0.11]. However, higher subjective SES significantly led to stronger BJW, b = 0.12, $t(112) = 4.12$, $p < .001$, 95% CI = [0.06, 0.18].

As shown in Figure 2, subjective SES moderated the general money effect on just-world beliefs: Even though significance did not quite reach conventional levels, the first

interaction revealed that the effect of subjective SES was stronger in the money conditions compared to the control condition, $b = 0.08$, $t(112) = 1.87$, $p = .06$, 95% CI = [0.00, 0.15], $f^2 = .03$, [0, 0.08], while the second interaction showed that the two money conditions did not differ significantly, $b = 0.02$, $t(112) = 0.46$, $p = .65$, 95% CI = [-0.06, 0.09].[4]

Simple slope analysis revealed that the higher participants' subjective SES the more they believed in a just world, both when reminded of their own money, $b = 0.18$, $t(112) = 3.23$, $p = .002$, 95% CI = [0.07, 0.29], as well as when reminded of other people's money, $b = 0.14$, $t(112) = 2.95$, $p = .004$, 95% CI = [0.05, 0.24]. Whereas in the control condition participants' subjective SES did not significantly affect their just-world beliefs, $b = 0.05$, $t(112) = 0.99$, $p = .32$, 95% CI = [-0.05, 0.14]. Hence, similar to Study 1, subjective SES marginally moderated the effect of money on the endorsement of the current system. Whether the prime activated thoughts of "my" or "their" money did not substantially alter the results. This implies that both manipulations evoked similar tendencies to act in accordance with one's self-interest.

[4] When applying a more liberal dropout criterion that comprised participants who interrupted the study ($n = 4$; >= 22.0 min, i.e. upper quartile + 1.5 interquartile range) and non-native speakers (2) the first interaction was significant , $b = 0.09$, $t(106) = 2.15$, $p = .03$, 95% CI = [0.01, 0.18].

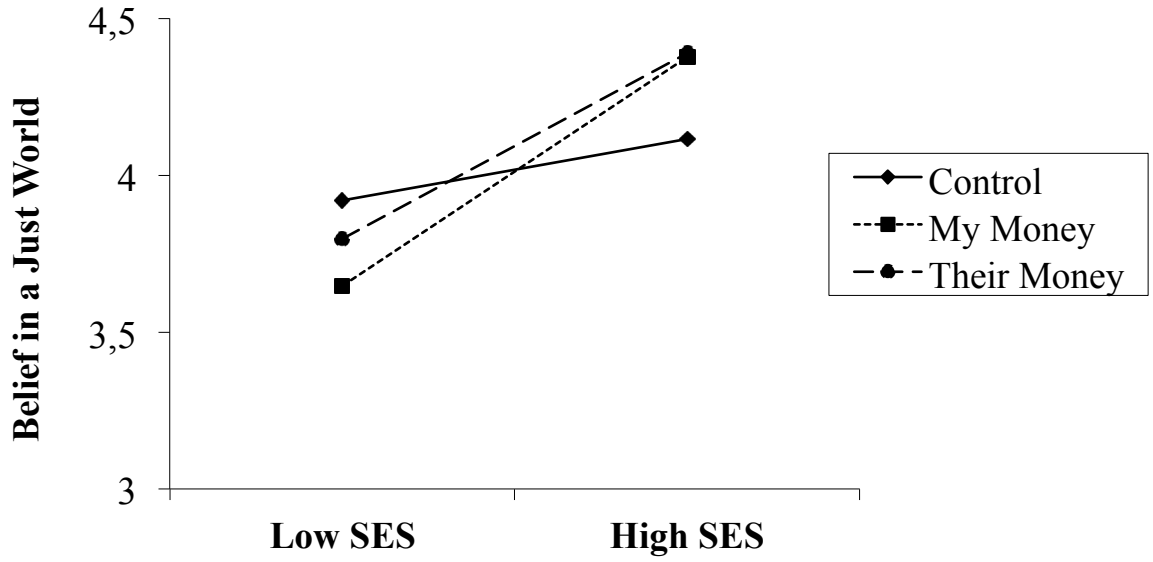

Figure 2. Estimated Belief in a Just World scores by condition (Control vs. My Money vs. Their Money) for lower (1 *SD* below the mean) and higher (1 *SD* above the mean) subjective socioeconomic status (SES; 1 - 10).

Meta-Analysis

We conducted a small-scale meta-analysis (Cumming, 2012) to assess the robustness of the interaction effect of money and subjective SES on system justification. In a meta-analysis, the pooled estimate *g* is a far more trustworthy indicator for the true effect, since it is based on the entirety of the data (Braver, Thoemmes, & Rosenthal, 2014). Hence, we also included a third study, which was equivalent with Study 1 but with the Belief in a Just World Scale (Rubin & Peplau, 1975) as a dependent variable (Appendix D for details). We do not report that study individually in this paper, because the manipulation check indicated a comparably small effect of the manipulation (d = .32, 95% CI = [-0.04, 0.68], $t(121) = 1.77$, p = .08; see Study 1: d = .62). Presumably due to the less powerful manipulation, the interaction between money and subjective SES on BJW was not significant, $b = 0.05$, $t(114) = 0.92$, p = .36, 95% CI = [-0.06, 0.16], f^2 = .01, [0, 0.03]. Even though the premises to find the expected interaction in this study were probably not equivalent, we nevertheless decided to incorporate the data to follow Braver et al.'s (2014) logic of cumulating evidence.

According to this logic, a meta-analysis should include all studies because a greater sample size provides a better indicator of the true effect than each study by itself.

To calculate the effect size for the interaction effects, we computed *f*-square of the interaction terms from the three studies and converted them into Hedges' *g* as an unbiased estimate of the population effect size (Cumming, 2012, pp. 281-320). By using a weighted random-effects model, each experiment was weighted depending on the variance of its effect size. Moreover, this procedure enables to test, whether the discovered interaction effects in our studies were heterogeneous. *Q*, as a measure for heterogeneity, therefore indicates whether the effects obtained across the studies differ due to sampling variability of the same underlying effect or whether the studies are based on heterogeneous effect sizes (Borenstein, Hedges, Higgins, & Rothstein, 2009).

Our meta-analysis revealed that, across all three experiments the interaction effect was significant, $g = 0.32$, $p = 0.002$, 95% CI [0.11, 0.53]. Thus, although the manipulation in our third study was presumably too weak to cause a significant interaction effect with subjective SES, data of all three studies provide cumulative evidence for the interaction effect in the meta-analysis (see Braver et al., 2014). The effect was not heterogeneous across the three experiments, Q ($df = 2$) = 1.27, $p = 0.53$. This suggests, that we found similar effects across our three studies or, according to Braver et al. (2014), that the predicted interaction effect replicated across three studies.

Even though this meta-analysis across 365 participants provides substantial evidence for the predicted interaction, the inclusion of our third study remains debatable. One might argue, that due to the non-significant manipulation check, we cannot confirm that thoughts of money had been reliably activated in the money condition. This dichotomous logic implies that Study 3 should be excluded from our analysis. A meta-analysis that consisted only of Study 1 and 2 ($N = 242$), still revealed a significant interaction effect, $g = 0.41$, $p = 0.002$,

95% CI [0.15, 0.66]. The variability of the effect size was not significant, Q (df = 1) = 0.16, p = 0.69.

General Discussion

Two experiments showed that participants' subjective SES moderates the effect of money reminders on the endorsement of the current socioeconomic system. When being reminded of money, participants with higher subjective SES tended to justify the existing socioeconomic system in the United States more strongly and believed more in the justness of its social outcomes than participants with lower subjective SES. A small-scale meta-analysis that included three experiments confirmed the general effect on the endorsement of the socioeconomic system and indicated no significant heterogeneity in the obtained effects.

We suggest that money cues do not generally lead to an increased approval of the current socioeconomic system, but mainly for those who profit from the respective system. Three reasons may contribute to this. First, being reminded of money might make the economic aspects of a system more salient and more accessible as a base for the judgment. Second, thinking of money causes participants to more strongly focus on their own personal advantage (e.g., Gino & Mogilner, 2014; Kouchaki et al., 2013; Reutner & Wänke, 2013). Third, reminders of money lead to a focus on the self and should therefore make system justification less likely when it conflicts with self-interest (Jost et al., 2003). As a result, money-reminded participants base their judgments about the social and economic system more strongly on whether they benefit from this system or not. Those who do well in the system will show increased support and increased belief in its fairness whereas those who sense that they have fallen behind will have a more critical view of the justness and legitimacy of the current system.

Although our results show consistently that subjective SES moderates the effect of money priming on the endorsement of the socioeconomic system, the correlational nature of the data pose a potential limitation. Only a manipulation of subjective SES, without activating the concept of money, would allow conclusive causal assumptions. A second shortcoming of this research is that we did not vary the type of money prime. Even though, the money scrambled sentence task is consistently used in the money priming literature (e.g., Boucher & Kofos, 2012; Caruso et al., 2013; Hansen, Kutzner, & Wänke, 2013; Jiang, Chen, & Wyer Jr, 2014; Kouchaki et al., 2013; Vohs et al., 2006), we cannot rule out the possibility, that the task activates alternative concepts such as economic inequalities. If so, then large parts of the research on money would be confounded with the activation of economic inequalities, which in turn would be possibly moderated by subjective SES. However, we find this to be unlikely. Assessing subjective SES before the manipulation (as in one condition of Study 1), should have activated thoughts of economic inequalities even in the control condition and therefore offset any effect of the descrambling task. This was not the case; we also found the interaction effect when we only considered those participants who reported their subjective SES before the manipulation, $b = 0.14$, $t(57) = 2.20$, $p = .03$, 95% CI = [0.01, 0.26].

It is also worth noting, that while corroborating studies that show effects of self-interest (Brown-Iannuzzi et al., 2015), our results seem to be at odds with some findings on system justification theory in which economically disadvantaged groups would be (at least moderately) more motivated to justify the socioeconomic system (see: Hunt, 2000; Jost et al., 2003). In our studies however, participants' subjective SES in the control condition was slightly positive related to system justification ($r = .12$) and BJW ($r = .15$), which established a rather conservative test for our moderation hypothesis. This positive relationship is not surprising, since an inverse relationship between SES and system justification depends on

many boundary conditions (Kay & Friesen, 2011) and is more likely when measured with unobtrusive measures that are embedded in large surveys (Jost, Pelham, & Carvallo, 2002; Jost et al., 2003) and when motives of self-interest are not salient (Jost, Banaji, & Nosek, 2004; Jost et al., 2003). In our studies system justification was assessed with explicit measures that were not embedded in other items. Consequently, Jost et al. (2004, p. 910) argue, it would be unwarranted to claim "that members of disadvantaged groups will always or even typically exhibit stronger support for the system than will members of advantaged groups".

Besides proposing a fresh look on money priming by suggesting that the effects may differ substantially depending on who is primed, as a side effect, our findings also offer conceptual implications for the recent debates about the replicability of money priming and perhaps priming in general (e. g. Asendorpf et al., 2013; Cesario, 2014; Locke, 2015). On the one hand, looking at the overall pattern, the present research further shows that the findings from Caruso and colleagues (2013) are difficult to replicate (see: Klein et al., 2014; Rohrer et al., 2015). On the other hand our research identified a moderator that may account for the effect found by Caruso et al. (2013, Study 1 & 2) if we assume that their participants were of relatively high subjective SES (students of an elite US university with renowned prestige, education and excellent job perspectives) compared to the replication samples. Note however, that such tentative conclusions are only based on three conceptual replications and an educated guess about the subjective social standing of Caruso et al.'s participants. On a different note we also point to the variance in our manipulation checks, which suggests that procedures of priming money may not reliably activate thoughts of money. The robustness of the priming procedure is however different from the robustness of the effects given the prime works and both should be treated separately when evaluating research on money priming.

Be that as it may, more generally, we suggest not merely to look for whether an effect appears and replicates, but in the interest of scientific advance to strictly test the predictions that derive from theory and understand the conditions that moderate its occurrence (Cesario & Jonas, 2014; Locke, 2015; Wheeler & Berger, 2007). Interindividual differences such as subjective SES may help to account for controversial findings in the field and expand our understanding of the psychological effects of money in general. Interestingly, the mental activation of money may not necessarily lead to the same behavioral outcomes and judgments for different people. While being reminded of money may make the privileged endorse the status quo, it may cause the less privileged to challenge this status and question the foundations of society.

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Appendix A

Original Descrambling Task (Vohs et al., 2006)

Instructions:

"The following test is part of an international project about playful methods for learning a language, provided by the Institute of Cognitive and Linguistic Sciences.

Your task is to descramble the provided words in order to create a meaningful sentence

Please write one correct sentence using **ONLY 4** of the **5** words in each line.

For example: **went earlier she ~~word~~ swimming = she went swimming earlier"**

Table A1.

Phrases used in the original descrambling task.

Phrase	Control Group	Experimental Group
1	you held pencil building the	you held pencil building the
2	on printer grass she walked	received a raise blue she
3	took tight he a glass	I a cashed pen cheque
4	to she music listened jump	to she music listened jump
5	metal I wrote letter the	metal I wrote letter the
6	ski she to wanted many	has the capital line he
7	opens he door his top	received they large city profits
8	we later will mountain swim	we later will mountain swim
9	is green the sweater bottom	revenues our rising book are
10	you coming are here purple	is green the sweater bottom
11	camping ten went girls book	hundred bill one bottle dollar
12	loves she skirt silky her	you coming are here purple
13	to car need we talk	camping ten went girls book
14	sky went gray the is	won green the I lottery
15	meal she the calendar ate	he wealthy is cup very
16	again late worked watch we	is hard he win studying
17	gift he the helping gave	secure I words financially am
18	paper long going was the	sky went gray the is
19	is outside cold desk it	pockets he deep blue has
20	dishes we washed song the	we cup afford can it

Phrase	Control Group	Experimental Group
21	room dark the city is	again late worked watch we
22	we coffee for went white	finances he manages mouse well
23	walked the keyboard dog she	paper long going was the
24	exam was the grass challenging	is outside cold desk it
25	up the stadium pick book	liberally money she paperclip spends
26	was fun outside party the	on printer grass she walked
27	is making sun dinner who	job well pays the arrow
28	read she paper the light	took tight he a glass
29	deep the water number is	salary paying high desk a
30	volume turn the flower up	opens he door his top

Note. Money phrases are displayed on a grey background.

Appendix B

Modified Descrambling Task

Instructions:

"The following test is part of an international project about playful methods for learning a language, provided by the Institute of Cognitive and Linguistic Sciences.

Your task is to descramble the provided words in order to create a meaningful sentence

Please write one correct sentence using **ONLY 4** of the **5** words in each line.

For example: **went earlier she ~~word~~ swimming = she went swimming earlier"**

Table B1.

Phrases used in the modified descrambling task.

Phrase	Control Group	"My-money Group"	"Their-money Group"
1	you held pencil building the	you held pencil building the	you held pencil building the
2	on printer grass she walked	received a raise blue I	received a raise blue she
3	took tight he a glass	We a cashed pen cheque	Peter a cashed pen cheque
4	to she music listened jump	to she music listened jump	to she music listened jump
5	metal I wrote letter the	metal I wrote letter the	metal I wrote letter the
6	ski she to wanted many	have the capital line we	has the capital line he
7	opens he door his top	received I large city profits	received they large city profits
8	we later will mountain swim	we later will mountain swim	we later will mountain swim
9	is green the sweater bottom	revenues my rising book are	revenues their rising book are
10	you coming are here purple	is green the sweater bottom	is green the sweater bottom
11	camping ten went girls book	**I the team bottle sponsor**	**He the team bottle sponsors**
12	is hard he win studying	you coming are here purple	you coming are here purple
13	bill the going sent we	camping ten went girls book	camping ten went girls book
14	sky went gray the is	won green the we lottery	won green the Mary lottery
15	meal she the calendar ate	I wealthy am cup very	he wealthy is cup very
16	again late worked watch we	is hard he win studying	is hard he win studying

Phrase	Control Group	"My-money Group"	"Their-money Group"
17	gift he the helping gave	secure we words financially are	secure they words financially are
18	paper long going was the	sky went gray the is	sky went gray the is
19	is outside cold desk it	pockets I deep blue have	pockets he deep blue has
20	dishes we washed song the	We cup afford can it	They cup afford can it
21	room dark the city is	again late worked watch we	again late worked watch we
22	we coffee for went white	finances I manage mouse well	finances he manages mouse well
23	walked the keyboard dog she	paper long going was the	paper long going was the
24	exam was the grass challenging	is outside cold desk it	is outside cold desk it
25	up the stadium pick book	liberally money I paperclip spend	liberally money she paperclip spends
26	was fun outside party the	on printer grass she walked	on printer grass she walked
27	is making sun dinner who	**job well pays my arrow**	**job well pays his arrow**
28	read she paper the light	took tight he a glass	took tight he a glass
29	deep the water number is	**salary is high desk my**	**salary is high desk his**
30	volume turn the flower up	opens he door his top	opens he door his top

Note. Money phrases are displayed on a grey background. Bold phrases are new scramble sentences creations in order to produce phrases with personal pronouns.

Appendix C

Word Stem Completion task (Vohs et al., 2006)

Instructions:

"Completion task

In this task we ask you to complete each word stem in order to create meaningful words.

Please answer as fast and intuitively as possible.

Example: "li" could be completed as "life" or "little". "

Table C1.

Items and possible answers of the word stem completion task.

Word Stem	Completion	Other Possible Completions
Sp	**Spend**	Spider, spinach, spine, sprawl
Ri	**Rich**	Right, rifle, ring, ridge
Ca	**Cash**	Caterpillar, cat, cab, candle
Co	**Coin**	Condition, cord, cobweb, cool
mon	**Money**	Monday, monsoon, monster
Fort	**Fortune**	Fortress, fortnight, fortitude
Wea	**Wealth**	Weather, wear, weapon
Nat	Nature	Natural, native, nationality
Ho	Home	Homework, house, hold
Wi	Window	Winter, wild, with
hea	Heart	Heat, heater, hear
Tr	Tree	Treasure, try, trouble, trespass
Sh	Shoe	Shower, show, shade
Dr	Drink	Drug, drag, drop
Bot	Bottle	Bottom, botanical, bothersome

Note. Word stems and completions related to money are put in bold.

Appendix D

Study 3

Design. This study was identical with study 1 and executed at the same time, except that we used the Belief in a Just World Scale (Rubin & Peplau, 1975) instead of the System Justification Scale as a dependent variable. From 150 participants five left no data and 22 did not follow the instructions to create four-word phrases in the manipulation. This resulted in 123 participants.

Materials and procedure. The procedure was equivalent to study 1. The 20-item Belief in a Just World Scale as an adapted version by Caruso et al. (2013; 1 = strongly disagree; 7 = strongly agree) resulted in a reliability of $\alpha = .66$.

Results. A t-test revealed that money-reminded participants created marginally significant more money words ($n = 60$, $M = 1.12$, $SD = 1.09$) than when not reminded of money ($n = 63$, $M = 0.81$, $SD = 0.82$), $t(121) = 1.77$, $p = .08$, $d = 0.32$, 95% CI = [-0.04, 0.68]. This effect was smaller than previous studies using this method (Vohs et al., 2006) and not -moderated by subjective SES, $b = -0.04$, $t(118) = 0.41$, $p = .68$, 95% CI = [-0.23, 0.15].

To test our moderation hypothesis, we regressed system justification on the prime (-1 = control, 1 = money; mean centered), subjective SES (1 – 10; mean-centered), SES-order (-1 = before manipulation, 1 = after dependent variable; mean-centered) and all interaction terms. For participants with an average value in subjective SES, the money prime ($M = 3.97$, $SD = 0.63$) did not lead to stronger just-world beliefs than in the control condition ($M = 4.04$, $SD = 0.44$), $b = -0.07$, $t(114) = 0.73$, $p = .47$, 95% CI = [-0.27, 0.12]. Subjective SES significantly affected participants' system justification, $b = 0.03$, $t(114) = 1.27$, $p = .21$, 95% CI = [-0.02, 0.09] with people higher in subjective SES reporting higher system justification. The expected interaction of money and subjective SES did not significantly predicted just-world

beliefs, $b = 0.05$, $t(114) = 0.92$, $p = .36$, 95% CI = [-0.06, 0.16], $f^2 = .01$, [0, 0.03].[1] This interaction was not affected by SES-order. Neither the three-way interaction, $b = 0.01$, $t(114) = 0.11$, $p = .91$, 95% CI = [-0.20, 0.23], nor the two-way SES-order x Prime, $b = 0.08$, $t(114) = 0.39$, $p = .70$, 95% CI = [-0.31, 0.47], SES-order x Subjective SES, $b = -0.01$, $t(114) = 0.15$, $p = .88$, 95% CI = [-0.12, 0.10], or the effect of SES-order were significant, $b = -0.02$, $t(114) = 0.25$, $p = .81$, 95% CI = [-0.22, 0.17]. Collapsing over SES-order, simple slope analysis (Hayes, 2013) revealed neither a significant effect of subjective SES on system justification in the control condition, $b = 0.01$, $t(118) = 0.31$, $p = .76$, 95% CI = [-0.06, 0.08] nor in the money condition, $b = 0.06$, $t(118) = 1.57$, $p = .12$, 95% CI = [-0.02, 0.14].

[1] When applying a more liberal dropout criterion that comprised participants who interrupted the study ($n = 4$; >= 24.0 min, i.e. upper quartile + 1.5 interquartile range) non-native speakers (6), participants who reported to be distracted (2) and one person who reported to have been "wrongfully injured by a corrupt legal system" the interaction was still not significant , $b = 0.07$, $t(102) = 1.19$, $p = .24$, 95% CI = [-0.05, 0.19].

Does Money Change Political Views? - An Investigation of Money Priming and the Preference for Right-Wing Politics

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Abstract

Former research suggested that money primes might affect people's political orientation. In a multistudy approach across seven studies we explored this research question. Throughout the studies we used different dependent variables and samples and combined the results in a small-scale meta-analysis to test two competing hypotheses. Independent of the measures and experimental setting, our findings did not indicate that money primes lead to stronger right-wing orientations (hypothesis 1). The results for our alternative hypothesis that the effect is moderated by subjective socioeconomic status revealed some empirical support for an interaction effect and showed marginal significance. These findings suggest that, contrary to former research and theoretical predictions, the money priming effect on political orientation is most likely small and dependent on one's subjective socioeconomic status. Possible implications for money priming research and political psychology are discussed.

Keywords

Money, priming, political orientation, meta-analysis

Introduction

Money is a central cue in our everyday life. In the modern western world it is barely possible not to get in touch with money or money-related cues. In this regard it seems somewhat concerning that research has shown that subtle reminders of money can have tremendous effects on people's behavior and attitudes across different cultural backgrounds and age groups (for review see: Vohs, 2015). Regardless of whether participants are Asian, European or American, when being primed with money, they tend to work more persistently on tasks (Boucher & Kofos, 2012; Vohs, 2015; Vohs, Mead, & Goode, 2006) and focus more on their own personal needs and goals (Vohs et al., 2006; Vohs, Mead, & Goode, 2008). However this emphasis on the personal self comes along with less concern about others. Subjects reminded of money have shown to be less inclined to help others (e.g., Vohs et al., 2006) and less susceptible to others' needs (Gino & Mogilner, 2014; Kouchaki, Smith-Crowe, Brief, & Sousa, 2013; Reutner & Wänke, 2013). While it seems that being subtly reminded of money is beneficial for the personal self, it is bad for the social self (Vohs et al., 2008). Thus, not surprisingly, money priming can also affect attitudes about the societal structure. Caruso, Vohs, Baxter, and Waytz (2013) found that U.S.-participants reminded of money were more likely to justify the existing system, belief in a just world, accept social inequality and dominance, and to endorse free-market systems.

It should be noted that these beliefs, the endorsement of free-market systems, the justification and the belief in the existing system, and the acceptance of social inequality and social dominance are deemed as underlying core beliefs of political right-wing orientation (Jost, Blount, Pfeffer, & Hunyady, 2003; Jost, Glaser, Kruglanski, & Sulloway, 2003; Jost & Hunyady, 2005). Also suggesting the notion of a parallel between a political right-wing orientation and the effects of being reminded of money, right-wing politics value personal effort and achievement (Caprara, Schwartz, Capanna, Vecchione, & Barbaranelli, 2006;

Feldman, 2013; Goren, Federico, & Kittilson, 2009), as do participants who were reminded of money (Boucher & Kofos, 2012; Vohs, 2015; Vohs et al., 2006, 2008; Zedelius, Veling, & Aarts, 2013). Analogously the diminished concern for others' needs stands in contrast to left-wing politics, which typically defend social egalitarianism, justice and social support for other's needs (Edlund & Pande, 2002; Jæger, 2008; Roemer, 1998). Hence, given that money primes can change people's underlying political beliefs would they also affect people's general political orientation *(*also termed *political ideology*; see Jost, 2006)?

The Present Research

In contrast to a person's political beliefs or attitudes, the general political orientation represents a person's placement on a political dimension (*left-right*; Piurko, Schwartz, & Davidov, 2011) and reflects a broader range of motivational and attitudinal aspects than the previously assessed political core beliefs (Caruso et al., 2013; Klein et al., 2014). In other words, while underlying political beliefs usually reflect content-specific attitudes, the general political orientation is affected by a wide range of different factors such as social identity, personality, values, beliefs, and situational influences (e.g., Brewer, 2001; Crawford, Brady, Pilanski, & Erny, 2013; Jost, 2006; Kandler, Bleidorn, & Riemann, 2012; Keefer, Goode, & Van Berkel, 2015). However, even though the political orientation of a person depends on many different factors, it is not invariant. Previous studies have shown empirical evidence that subtle contextual primes can activate certain norms and concepts and have powerful effects on political judgments (Berger, Meredith, & Wheeler, 2008; Burger & Bless, 2016; Druckman, 2004; Weinberger & Westen, 2008) and on people's general political orientation (Brown-Iannuzzi, Lundberg, Kay, & Payne, 2015; Carter, Ferguson, & Hassin, 2011). For example, Helzer and Pizarro (2011) have shown that the mere presence of a hand-sanitizer dispenser in a hallway is sufficient to provoke changes in participants' general political orientations reported on a field-survey.

Therefore, in the present research we aimed to thoroughly test whether money primes - beyond changing certain underlying right-wing beliefs - would also affect participants' general political orientation. Such an effect on political orientation could have wide-ranging implications and would render money primes a considerable subtle instrument for right-wing party campaigns. Nowadays, governments put an increasing effort in eliminating undue influence on voting (cf. Berger et al., 2008), while increasing evidence shows that subtle primes can be successfully applied in voting campaigns (Druckman, 2004; Weinberger & Westen, 2008) and can alter people's political judgments in quite a long-lasting way (Carter et al., 2011). But political campaigns are not the only possible source where money cues might substantially alter people's political orientation. Due to the ubiquity of money cues in people's lives that show no signs of waning (Vohs et al., 2008), money could – even if only for a short instant - unobtrusively affect, whether one signs a political petition, reads certain information from a political party, or simply to which political opinion a person pays attention on Facebook.

In sum, we sought to test in an experimental setting, whether money primes would lead participants to change their self-reported political orientation and their preferences for political parties, which could – in its most extreme consequence - even alter people's voting behavior in real life. While so far our argument suggested a possible main effect, there is also evidence suggesting a more complex effect. Both hypotheses are outlined as follows.

Main-effect-hypothesis. Previous research across Western countries suggested that some of the core beliefs of political right-wing orientation such as the acceptance of social inequality, social dominance orientation, the justification of free market systems, and the conviction that only personal effort and achievement should be rewarded, are susceptible to subtle cues of money (Boucher & Kofos, 2012; Caruso et al., 2013; Vohs, 2015; Vohs et al., 2006, 2008; Zedelius et al., 2013). Thus, one may hypothesize that reminders of money subtly

shift participants' political orientation to the right, because money primes change the beliefs and values on which it is built on.

Moderation-hypothesis. During the course of this research, the assumption that money generally triggers right-wing beliefs had been challenged. Schuler and Wänke (2016) reasoned that if reminders of money lead to a focus on the self and one's personal advantages (e.g., Gino & Pierce, 2009; Reutner & Wänke, 2013; Vohs, 2015; Vohs et al., 2006), one should favor a societal system also more to the extent that it serves one's personal needs. In other words, when money - as hypothesized - stimulates people to focus more on their personal benefits, the justification of a system should also be more in accordance with one's personal interests compared to the necessities of others. Accordingly, the authors showed that when being primed with money, U.S.-participants who felt socioeconomically privileged showed more support of the U.S.-American societal system that favors members from the upper levels of society, compared to participants who felt socioeconomically disadvantaged. In line with this reasoning, one would hypothesize that reminders of money lead to an elevated focus on one's personal needs which in turn influences participants' political orientation according to their perceived standing in the social hierarchy (i.e. subjective socioeconomic status [subjective SES]) as well. When being primed with money, only those who believe they would actually benefit from its policies and the general ideology that comes along with it, should endorse right-wing politics. In other words, when considering oneself at the top of the social hierarchy (higher subjective SES), right-wing politics, which – by tendency – aim to preserve the social order and criticize social egalitarianism, redistribution, and equal chances (Giddens, 1998; Jost, Glaser, et al., 2003; Roemer, 1998), are more in line with one's personal interests (see Brown-Iannuzzi et al., 2015). For people who see themselves more at the bottom of society (lower subjective SES) this benefit is less clear. On the one hand, they should favor left-wing politics when inclined to pursue their personal interests through money primes. Left-wing politics seek to minimize social hierarchies,

support the interests of the disadvantaged, and fight for inclusion, redistribution, and equal rights (Edlund & Pande, 2002; Jæger, 2008; Shen & LaBouff, 2016) and are therefore supported by people with a lower subjective SES (Brown-Iannuzzi et al., 2015). On the other hand, people with a lower subjective SES do not necessarily believe that left-wing politics would be favorable for them (e.g., Jost, Pelham, Sheldon, & Ni Sullivan, 2003). Still, overall priming effects should be more pronounced for those subjectively higher in the social hierarchy and therefore moderated by subjective SES.

Methodological Approach

To perform a rigorous test of the hypothesis that money primes affect political orientation, we adopted a multistudy approach using different dependent variables throughout the studies. Such an approach is more informative than the test of a single paradigm (Tuk, Zhang, & Sweldens, 2015) and allows examining whether a possible underlying effect is restricted to a certain measure or whether a possible effect generalizes across several measures. Because the moderation-hypothesis seemed a plausible alternative to the main-effect-hypothesis, we included subjective SES as a possible moderator for the second half of our studies and conducted further studies online to increase the samples' variance on subjective SES.

To overcome power problems in single studies (Cumming, 2014; Tuk et al., 2015), we conducted a small-scale meta-analysis (Braver, Thoemmes, & Rosenthal, 2014; Goh, Hall, & Rosenthal, 2016) across our studies. The analysis is based on more data, has smaller confidence intervals for the estimated effect size and is thus more reliable in detecting whether an effect exists or not (Cumming, 2012). Moreover, by applying a random-effects model, the internal meta-analysis can indicate whether the obtained effect sizes represent the same or different underlying effects and can – depending and limited by the number of included studies – help to detect possible moderators across studies (Borenstein, Hedges, Higgins, & Rothstein, 2009). Because tests for heterogeneity and moderators tend to be

underpowered with small amounts of studies (k < 20; Huedo-Medina, Sánchez-Meca, Marín-Martínez, & Botella, 2006), we set the alpha level for both tests to .10 (Tuk et al., 2015). Furthermore and as an additional criteria for heterogeneity that is independent from the included number of studies, we applied $I^2 > 50\%$ as a criterion for heterogeneity (Braver et al., 2014).

In the present research, we present data of seven studies. Because we used different dependent variables to test our hypotheses and the settings slightly varied from study to study (i.e. we performed conceptual replications), it is possible that the true underlying effect sizes differ. Thus, we used a random-effects model in our meta-analysis as the generally more appropriate method of analysis (Borenstein, Hedges, Higgins, & Rothstein, 2010; Cumming, 2012). We first provide a general description of our samples and dropout criteria and then proceed with descriptions of the manipulation, the dependent variables and subjective SES as a possible moderator. After providing an overview of our studies, we test our two hypotheses and conduct some exploratory moderation analysis.

Method

The studies were either conducted in the laboratory or online. All studies had in common that participants worked on two ostensibly unrelated experiments. Across all studies the first experiment was a money priming manipulation that was framed as a language task on playful learning methods and comprised a descrambling sentence task (see Hansen, Kutzner, & Wänke, 2013). The second experiment was framed as a study on decision-making and contained one of the dependent variables. Minor variations between studies, such as whether demographic information was assessed at the beginning or at the end of the study, are described in Appendix A.

Participants and Dropouts

In all studies we exclusively recruited German participants to retain the same manipulation and to be able to relate studies where we used preferences for German political parties as the dependent measure with studies where we used self-reports of political orientation. Because the suggested psychological mechanisms of money priming seem to be universal and have been found across many different countries (see Vohs, 2015), we did not expect particular cultural differences.

For a total of seven studies (N = 666; Table 1) we applied one common dropout-criterion to make sure only those participants who were adequately primed with money, were included in the studies. Hence, we excluded participants who participated twice (33), participants who did not follow the instructions to create four-word phrases in the priming task (18), non-German native speakers (due to the language-based prime; 27) and participants who interrupted the study (i.e., upper quartile of study duration + 1.5 interquartile range; 16).[i] The remaining sample consisted of 572 participants (237 males, 2 other; M_{age} = 28.5, SD = 11.5).

Independent Variable and moderator

Descrambling Task. Participants were provided with 22 word-sets, each consisting of five words and had to descramble each set into a correct sentence of four words. In the experimental condition, 15 out of 22 word-sets contained words associated with money (e.g. money, salary, coin, cashbox). In the control group these words were substituted with money-unrelated words.

Subjective Socioeconomic Status (subjective SES). In order to assess subjective SES, participants completed a digital version of the MacArthur Scale (Piff, Kraus, Côté, Cheng & Keltner, 2010). An image of a 10-rung ladder was depicted as a representation of where people stand in society. While the top rung was labeled as representing those with the highest standing in the social hierarchy, the lowest rung was labeled as representing those with the

lowest standing in the social hierarchy. To measure subjective SES, participants were instructed to indicate on a 10-point scale where they would place themselves on this ladder relative to others in society (M = 5.94, SD = 1.75; only studies 4-7).

Dependent Variables

Self-report (A). As a measure for political orientation that we used in our first study, we applied a three-item measure that had been shown to be sensitive to environmental influences (Helzer & Pizarro, 2011). Participants were asked to which extent they identify themselves as fiscally, morally and socially conservative (1 = *very conservative*) or liberal (7 = *very liberal*). The inter-item correlation of the fiscal item was low (r_{moral} = .05; r_{social} = -.02), which was possibly due to the unclear meaning of the term fiscal conservatism among German participants.[ii] Hence, the reliability of the three-item scale was low, with a Cronbach's alpha of .48 (Cronbach's α = .65 in Helzer & Pizarro, 2011). The removal of the fiscal item raised the internal consistency to a Cronbach's alpha of .76. In consequence, we conducted the analysis with the two-item version of the scale and used different items for self-reported political orientation for subsequent studies.

Self-report (B). For the succeeding studies we used an item from a German Political Panel Study (Breyer, 2015) that reflects participants' self-placement on an inter-culturally robust left-right dimension (Piurko et al., 2011). To assess general political orientation ("Where would you place yourself if 1 was *left* and 11 was *right*?"), participants reported their political orientation on an eleven-point scale.[iii]

Political party preferences. As a complement to the self-report measures, we asked participants to indicate their preference for the six most popular political parties in Germany at the time (Die Linke, Die Grünen, SPD, AFD, CDU/CSU, FDP). To compute our dependent variable, each participant evaluated the six parties in a randomized order (1, "very negative" – 11, "very positive"). We also obtained ratings for the perceived general political orientation of

each party (1, “very left-wing” – 4 “very right-wing”) in an independent pretest (see Appendix B for details). To compute an index for political orientation, we then calculated Fisher’s z between the parties’ political orientation scores (assessed in the pretest) and participants’ evaluations of each party (assessed in each study). Correlation-scores above 0 indicate a more favourable rating for parties considered on the political right compared to parties considered left. Correlation-scores below 0 indicate the opposite. Our measure for party preferences consistently showed strong correlations with participants’ self-reported political orientation (assessed before the manipulation) in the control groups of each experiment (r_{range} = .54 - .65; see Appendix C for details). We thus used the obtained indices, which are typically strongly related to left-right self-placement measures (Van Deth & Geurts, 1989), as a reliable choice measure for political orientation.

Table 1
Studies Overview With Different Dependent Variables

#	$N_{included}$	*Study Type*	*DV*	*Moderator*
1	76	Lab	Self-report (A)	
2	104	Lab	Party preferences	
3	63	Lab	Party preferences	
4	70	Lab	Self-report (B)	SES
5	101	Online	Self-report (B)	SES
6	65	Online	Self-report (B)	SES
7	93	Online	Party preferences	SES

Note. $N_{included}$ = participants included in the analysis; *DV* = dependent variable; *Self-report* = self-report measures (A) and (B); *Party preferences* = preference measure for political parties; *SES* = subjective socioeconomic status.

Results

We meta-analyzed our studies by converting main effects and interaction effects into Pearson's correlation for comparability and ease of interpretation (see Appendix D for complete overview). All correlations were then transformed into Fisher's z for analysis. For our internal meta-analyses we applied a random-effects model (fitted with a restricted maximum-likelihood estimator for heterogeneity; see Viechtbauer, 2005), using the package METAFOR in R (Viechtbauer, 2010). The meta-analytic estimates and the respective confidence intervals were then converted back to Pearson's correlation coefficient for presentation throughout the text (cf. Goh et al., 2016).

The main-effect hypothesis

Overall, the small-scale meta-analysis on participants' right-wing orientation revealed a

non-significant effect in the opposite direction. As shown in Figure 1, money-primed participants did not report significantly higher right-wing orientations than participants not reminded of money, $N = 564$, $r = -.07$, 95% CI = [-0.17, 0.02], $Z = -1.57$, $p = .117$, $Q(6) = 7.00$, $p = .32$, $I^2 = 17.83\%$. The meta-analysis across all participants revealed similar results.[iv].

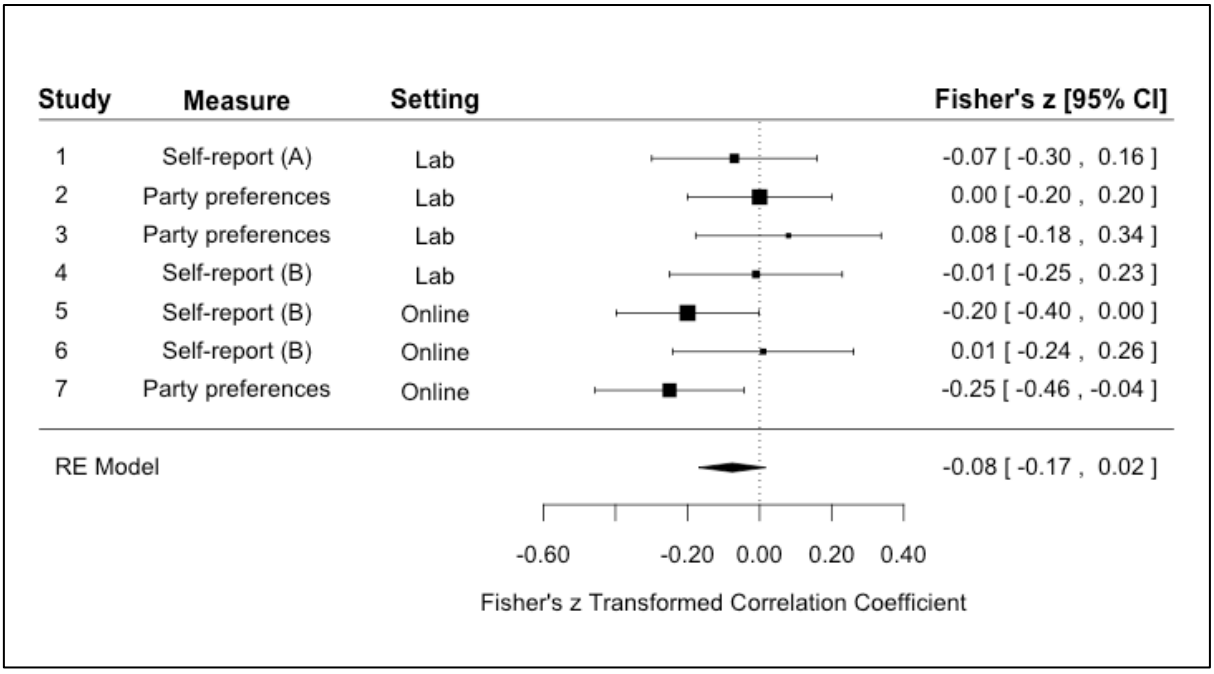

Figure 1. Forest plot of the effect of money priming on political orientation. Positive effects reflect stronger right-wing orientation after being reminded of money. For each included study, the measure type, the setting, Fisher's z value, and the corresponding 95% confidence interval (black lines) are reported. The effect sizes are illustrated with squares whose sizes are representing the relative weight of each study in the random-effects meta-analysis. The diamond depicts the effect size of the meta-analytic estimate and its 95% confidence interval.

The cumulative evidence of our seven studies did not support the hypothesis that being reminded of money leads to stronger right-wing preferences. Considering the small confidence intervals of the effect size estimate (for a detailed interpretation of confidence intervals see Cumming, 2012), one can conclude that the effect is most likely either essentially zero or small and in the opposite direction as predicted.

The interaction-effect hypothesis

To test the hypothesis that subjective SES moderates the effect of money priming on political orientation, we once again performed a small-scale meta-analysis. The meta-analysis (Figure 2) now revealed a marginally significant interaction effect in the expected direction, $N = 327$, $r = 0.14$, 95% CI = [-0.01, 0.28], Z = 1.83, $p = .068$, $Q(3) = 5.42$, $p = .14$, $I^2 = 42.97\%$.[v] Heterogeneity analysis indicated moderate but not disproportionate levels of heterogeneity.

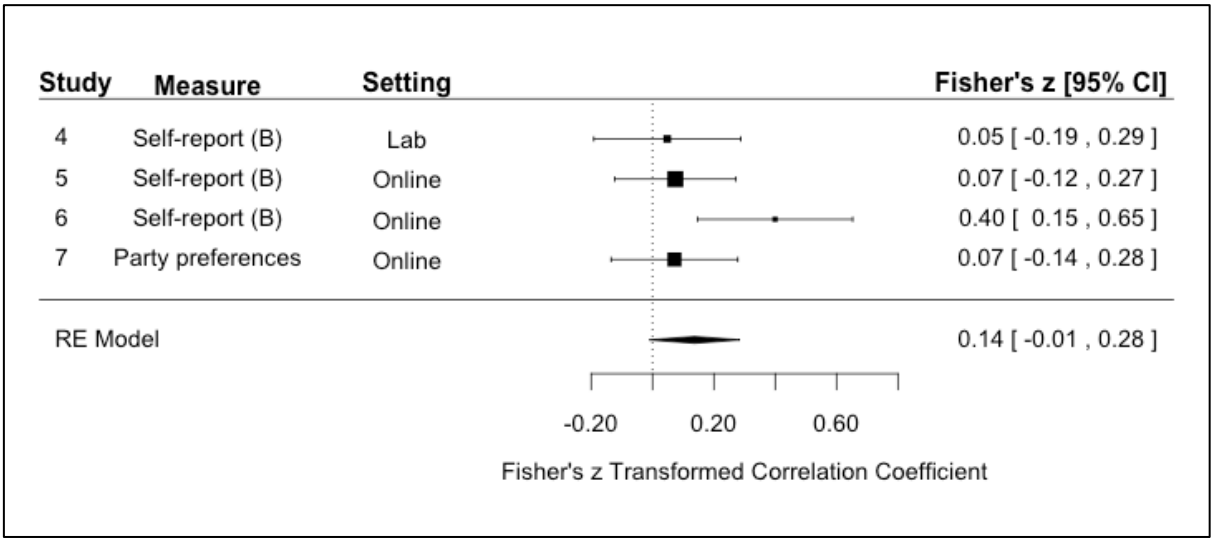

Figure 2. Forest plot of the interaction effect of money priming x subjective socioeconomic status (SES) on political orientation. Positive effects reflect stronger (weaker) right-wing orientation for participants with higher (lower) subjective SES after being reminded of money. For each included study, the measure type, the setting, Fisher's z value, and the corresponding 95% confidence interval (black lines) are reported. The effect sizes are illustrated with squares whose sizes are representing the relative weight of each study in the random-effects meta-analysis. The diamond depicts the effect size of the meta-analytic estimate and its 95% confidence interval.

Evidence across the four studies revealed that participants with higher subjective SES showed stronger and participants with lower subjective SES showed weaker right-wing orientations when being reminded of money compared to participants in the control condition (Figure 3).

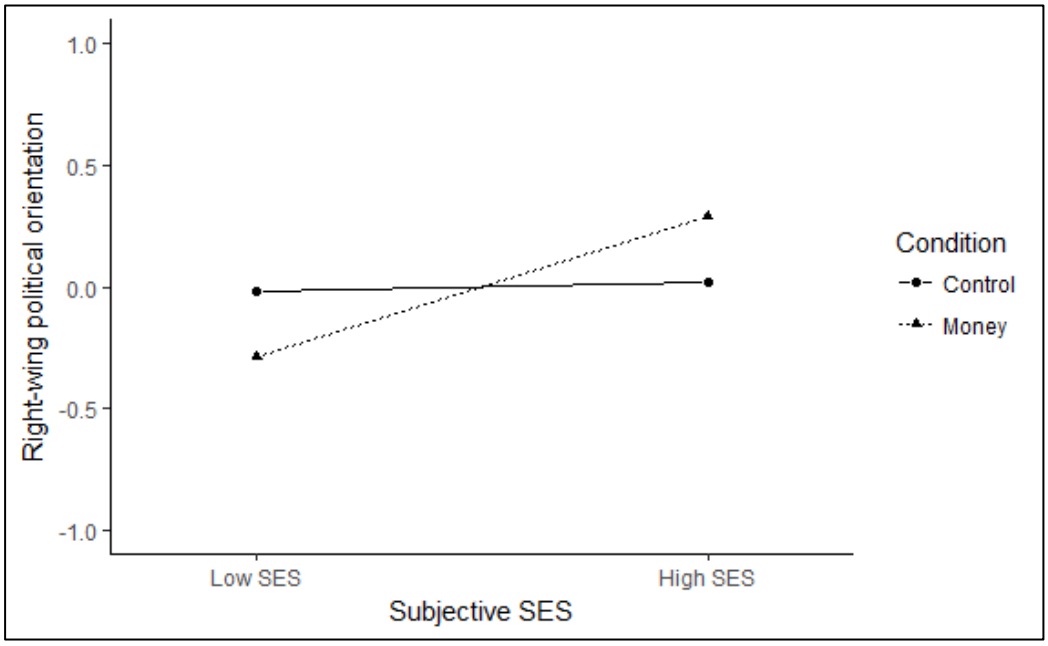

Figure 3. Synthesized simple slopes of control (β = 0.017) and money condition (β = 0.291) for studies 4 – 7. To allow for accumulation, beta regression coefficients were derived from z-standardized variables and synthesized with a weighted least squares approach (Becker & Wu, 2007). Values of right-wing political orientation above zero indicate stronger right-wing orientation and values below zero indicate stronger left-wing orientation for lower (1 *SD* below the mean) and higher (1 *SD* above the mean) subjective SES. SES = socioeconomic status.

The cumulative analysis on the direct measures seems to provide some tentative support for the interaction hypothesis. In contrast to the main effect hypothesis and in line with previous research (Schuler & Wänke, 2016), the cumulative evidence revealed a marginally significant effect for the moderation hypothesis. However, the effect is most likely small to medium sized and further studies would be needed to provide evidence for the effect.

Moderation analyses

Interestingly, the interaction effect money x subjective SES on the direct measures revealed moderate heterogeneity. Such heterogeneity is generally not surprising, because variability in priming effects across different samples and contexts should actually be

expected (Cesario, 2014). Past priming research has revealed a great variety of moderating factors (e.g., DeMarree, Wheeler, & Petty, 2005; Ma & Correll, 2011; Yi, 1993). Thus, many critical factors within the sample and the experimental design that might seem irrelevant can, if varied, eliminate or even reverse priming effects. Yet, even though such moderators clearly decrease the generalizability of the priming effect, they are of significant value for researchers. Priming research is still in its beginning with regard to the exploration of its processes and its process related variables. Thus, the discovery of moderating variables can provide meaningful information about the effect and the theory (Cesario, 2014).

To test for the variation in the interaction effect money x subjective SES between the studies, we applied a mixed-effects model (Borenstein et al., 2009) and explored two potentially relevant characteristics that could have caused some of the variance in our priming effects. First, different measures might have varied in their sensitivity to assess political orientation and thus possibly caused different effect sizes. However, whether we used self-reports (A & B) or the choice measure (party preferences) to assess political orientation did not significantly moderate the overall effect, QM [df = 1] = 0.10, p = .755. Second, our studies in the laboratory differed in many aspects from our online studies. Factors such as the elevated subjective SES among our laboratory samples (6.68 versus online: 5.82), being surrounded by in-group members in the laboratory (Fessler & Holbrook, 2013) or a difference in the intensity of the prime in the laboratory compared to the online studies could have potentially moderated the results. However, the study location did not show statistically significant differences in the effect sizes across studies, QM [df = 1] = 0.47, p = .493.

Even though our analyses did not reveal significant moderators, we cannot rule out the possibility that study location or the application of different measures moderate the effect. With few studies, the power to detect relevant moderators decreases and therefore the moderation analyses should be interpreted with caution (Borenstein et al., 2009).

Discussion

We considered the hypothesis that money primes change people's political orientation, an important question for basic research and for its relevance in real life, which was implied by several publications (e.g., Caruso et al., 2013; Vohs, 2015; Zedelius et al., 2013) and subsequently questioned by other researchers (Klein et al., 2014; Rohrer, Pashler, & Harris, 2015; Schuler & Wänke, 2016; Vadillo, Hardwicke, & Shanks, 2016).

From a basic research perspective, it was interesting to note that the predictions and empirical findings of money priming allow for two competing hypotheses. On the one hand, money primes evoke a focus on personal achievement and make people less sensitive to the needs of others (Vohs et al., 2008). Thus, because money priming triggers central convictions of right-wing politics, money primes should also make people endorse right-wing politics in general. On the other hand, reminders of money lead to a focus on personal concerns and one's personal advantages (see Gino & Pierce, 2009; Kouchaki et al., 2013; Reutner & Wänke, 2013; Vohs, 2015; Vohs et al., 2006). Thus, money priming should make people endorse a political orientation that increases their personal benefits. More specifically, when primed with money, people should favor right-wing politics when feeling socioeconomically privileged and less so when feeling socioeconomically disadvantaged. These two opposing predictions did not only emphasize the relevance to investigate this research question but suggest as well that a more specified theory of money priming might be warranted.

From an application perspective, the question whether subtle money primes could alter people's political orientation is of fundamental relevance. In western societies, money cues become rather abundant and could thus – as a repetitive prime – potentially provoke lasting changes in people's political orientation and ultimately voting behavior (cf. Carter et al., 2011). As a prerequisite, money primes would need to work in the field - not only in experimental settings – and would need to have long lasting effects. Recent research

demonstrates the efficacy of money priming in noisy real-life contexts such as coffee bars (Mogilner, 2010), on the street (Guéguen & Jacob, 2013), or in work environments (Mok & De Cremer, 2016) on daily behaviors. More importantly, Beus and Whitman (2015) suggest that money can have durable effects in work contexts, when made chronically salient. An effect of money cues, whether shortly before the voting booth or as a repetitive visual cue in political campaigns and daily contexts, would be rather troubling and calls for careful examination.

Hence, we adopted a multistudy approach to examine this question. Across seven studies, we found little evidence for a main effect of money priming on political orientation among German participants. Furthermore, for the alternative hypothesis that subjective SES moderates the priming effect on political orientation, we found tentative, marginally significant evidence for a small to medium size effect.

Before reasoning about the implications of these results, it is useful to consider possible shortcomings of this research. First, Caruso and colleagues (2013) found money priming effects on certain political beliefs only among U.S.-American participants, "because the key components of the experimental design were focused on America" (p. 304). Although our measures were not restricted to U.S.-participants, it is possible that money-priming effects on political orientation would reveal different results with US-participants due to cultural differences. However, we find this to be unlikely. Empirical support for the theoretical claim that money priming makes people more focused on themselves and their personal benefits has been shown across different countries and cultural backgrounds (Vohs, 2015). Furthermore, Schuler and Wänke (2016) tested U.S.-participants and found an interaction that concurs with our findings. Thus, although it remains an empirical question, we are not aware of a moderator in the field that would make different predictions for U.S.- and German participants regarding the suggested effect. Second, it is unclear how and whether these effects would generalize in noisy real-life environments, in which a myriad of different

factors drives people's actions (Mogilner, 2010). Even though previous research suggests that the effects of money priming can have significant effects in the field (e.g., Guéguen & Jacob, 2013; Mok & De Cremer, 2016), it remains an open question to which degree money primes could influence actual political orientation or voting behavior during an election.

Conclusions

Although more research is needed to conclude whether an interaction effect exists or not, our data provide some interesting conclusions. On the one hand, we found tentative evidence for the theoretically predicted interaction effect with subjective SES. After a decade of promising money priming research (Vohs, 2015) more and more researchers raise questions about the robustness, replicability, and credibility of the discovered effects and their theoretical framework (Klein et al., 2014; Pashler, Rohrer, Abramson, Wolfson, & Harris, 2016; Rohrer et al., 2015; Vadillo et al., 2016). Perhaps it is time to thoroughly search for moderators and limitations of money priming, and to advance the field and the theory for future research. Interestingly, it appears that a person's standing in the social hierarchy, which has become increasingly important for social psychology in general (Brown-Iannuzzi et al., 2015; Gruenewald, Kemeny, & Aziz, 2006; Keefer et al., 2015; Kraus, Côté, & Keltner, 2010), also plays a role for money priming (see also Schuler & Wänke, 2016) and might partly explain why money priming effects are generally not so robust. Hence, it seems worthwhile to keep this meaningful variable in mind when conducting future research with more representative or diversified samples in social and political psychology.

On the other hand, our goal was to examine whether the effects of money priming would have practical relevance for society. At first glance, our findings suggest that there is most likely little menace to the political processes in real life. Under relatively controlled settings, we found some tentative evidence for a small to medium money prime interaction effect, which might not hold in more unstable settings. It may well be that such priming effects are

smaller and much more limited than oftentimes assumed (Helzer & Pizarro, 2011; Weinberger & Westen, 2008) even though, depending on the context of the prime and further moderators, it is of course possible that the prime produces significant effects in real-life environments as has been indicated with similar environmental cues (Carter et al., 2011).

Accordingly, this research opened up some interesting lines for future research. A closer investigation of certain facets of political orientation such as its economic and social dimension (Ashton et al., 2005; Everett, 2013) could reveal that cues of money affect these concepts differently. Former research (Caruso et al., 2013) indicated that beliefs (e.g., social dominance orientation) associated with the economic dimension of political orientation (Jost, Federico, & Napier, 2009) were influenced by money priming. These findings fit with the general assumption that money primes would rather affect a dimension labeled compassion vs. competition (economic) than a dimension labeled moral regulation vs. individual freedom (social; cf. Ashton et al., 2005). Following a compatibility logic (Ajzen & Fishbein, 1977), this would imply that our dependent variables measured a broader concept (political orientation) and that measuring a sub-dimension would lead to stronger effects. Furthermore, considering the potential implications that unobtrusive money priming effects would have for elections, field experiments would be a useful complement to our research to examine whether an effect on political decisions and ideologies holds in this setting. While we do not claim that a single priming episode will change people's attitudes for good it may suffice at a given moment to sign a petition, or stop to take a leaflet or listen to a candidate's arguments. While these individual actions might not seem too influential on their own, taken together and over time they could tilt opinions in favor of certain political judgments.

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Appendix

See supplementary file

[i] Because we were performing a meta-analysis with a comparably large and diverse sample size, we were more concerned about making sure that all included participants were correctly primed rather than further increase statistical power. We thus applied a more liberal criterion.

[ii] With regard to fiscal policies the terms *conservative* and *liberal* are not well defined in Germany. For example, the liberal party in Germany (FDP) is advocating fiscally conservative policies despite their claim to be liberal. Thus, the item might have produced inconsistent responses.

[iii] Additionally we assessed two content related political attitudes on immigration and taxation that turned out to be unsuitable to assess general political orientation. Due to prominent political events at that time (Euro crisis & refugee crisis), the items indicated specific attitudes and revealed a questionable internal consistency when included to the general political orientation item (Cronbach's α range = .59 - .63; George & Mallery, 2003).

[iv] Across all participants, money-primed participants did not report significantly higher right-wing orientations than participants not reminded of money, $N = 653$, $r = -0.03$, 95% CI = [-0.13, 0.06], Z = -0.74, $p = .46$, $Q(6) = 8.28$, $p = .219$, $I^2 = 28.45\%$.

[v] The interaction of money and subjective SES did not significantly predict participants' right-wing orientation across all participants, $N = 365$, $r = 0.16$, 95% CI = [-0.03, 0.34], Z = 1.64, $p = .102$, $Q(3) = 9.65$, $p = .022$, $I^2 = 70.13\%$.

Appendix A

Detailed description of study procedures

Study 1. After initial instructions that told participants that they would work on two different experiments, they completed the *descrambling task*, which either contained money related words or not. After completion of our ostensible experiment 1, we asked participants about the purpose of the study. In experiment 2 they received further instructions (signed by a different researcher) and completed the *self-report A* as a dependent measure. Afterwards participants worked on an exploratory perception task, which was not relevant for our hypothesis. In the task, we presented ten faces of unknown male politicians and asked participants to indicate their voting preference for each of them. Thereby we wanted to investigate whether money primes would also affect the preference for warm or competent faces. After the perception task, we assessed demographic information and asked participants about the purpose of our ostensible experiment 2.

Study 2. First, participants provided demographic information and indicated their general political orientation on *self-report B*. Afterwards they completed the descrambling task. In the ostensible second experiment, participants were first asked to complete the perception task of Study 1 and then asked to complete the *party preference* measure as our dependent measure. Lastly, we asked participants about the purpose of the experiment.

Study 3. This study was a replication of Study 2 with the only exception that we did not assess the visual perception task between the manipulation and the dependent measure. As in Study 2 general political orientation (*self-report B*) was assessed before the manipulation.

Study 4. This Study was a replication of Study 1, with an identical procedure but a different dependent variable (*self-report B* instead of *self-report A*). Before providing demographic information, participants indicated their mood on a brief 3-item scale (the difference between the control group and the experimental group was not significant, $p = .997$) to control for mood effects. Additionally, subjective SES was assessed as part of the demographic information at the end of the study.

Study 5. This Study was a replication of Study 4 in which we recruited participants through the online platform CrowdFlower. In contrast to Study 4, mood was no longer assessed and subjective SES was assessed before the other demographic variables.

Study 6. This Study was again a replication of Study 4 in which we recruited participants online from various online study pools. In contrast to Study 4, mood was no longer assessed.

Study 7. This Study was a replication of Study 2 in which we recruited participants through the online platform CrowdFlower. In this study we did not assess the visual perception task between the manipulation and the dependent measure. As in Study 2 general political orientation (*self-report B*) was assessed before the manipulation. Additionally, we assessed subjective SES at the end of the study.

Appendix B

Pretest for dependent variable: Political party preferences.

Description of the sample. A total of 96 participants from Germany (34 males; M_{age} = 25.61, *SD* = 6.41) completed the questionnaire after they were recruited online through various message-boards on facebook.com. None were excluded before analysis. General political orientation was assessed with the same item used as the dependent variable *Self-reported political orientation (B)* ("Where would you place yourself if 1 was *left* and 11 was *right*?"). The sample was left-leaning (*M* = 4.68, *SD* = 1.64, Range 1-8).

Table A1
Political Parties and their left-/right-wing orientation, as rated by participants.

Political Party	*N*	*M (SD)*	*range*
Die Linke	95	1.09 (0.36)	1-3
Die Grünen	96	2.00 (0.46)	1-3
SPD	96	2.11 (0.43)	1-4
CDU	96	2.94 (0.48)	1-4
FDP	94	2.81 (0.61)	1-4
AFD	96	3.70 (0.58)	1-4

Note. All scales from 1 (left) – 4 (right). *N* = sample size, *M (SD)* = mean and standard deviation of respective ratings. *N* varies between parties, because some participants failed to provide complete ratings.

Appendix C

Table C1
Studies overview

Study	Total *N*	Included *N*	Age		Gender		Subjective SES		Party preference	
			M	*SD*	Male (%)	Female (%)	*M*	*SD*	*r*	*n*
Study 1	91	76	22.95	4.18	27.60	72.40	-	-	-	-
Study 2	120	104	21.14	2.56	28.00	72.00	-	-	.65***	48
Study 3	86	63	21.49	4.00	23.80	76.20	-	-	.63***	30
Study 4	73	70	22.77	4.42	24.30	75.70	6.37	1.40	-	-
Study 5	117	101	40.07	13.15	74.30	25.70	5.43	1.87	-	-
Study 6	77	65	26.71	7.25	28.10	70.30[a]	6.48	1.45	-	-
Study 7	102	93	38.91	11.85	68.80	30.10[a]	5.56	1.76	.54***	50

Note. Total *N* = number of participants; Included *N* = participants included in the analysis; subjective SES = subjective Socioeconomic Status; Party preferences = Pearson correlation in control groups between party preference measure and self-reported political orientation (assessed before manipulation). All descriptive statistics are based on the reduced number of included participants. Dashes indicate that no data was obtained for the respective cell.

[a] Missing percentage chose "other".

*$p < .05$. **$p < .01$. ***$p < .001$.

Appendix D

Table D1
Studies overview on obtained effect sizes

Study	Included *N*	CG *n*	EG *n*	DV CG *M*	DV CG *SD*	DV EG *M*	DV EG *SD*	Main effect *r*	Interaction effect *r*
Study 1	76	39	37	4.44	1.33	4.61	1.27	-.07	-
Study 2	104	51	53	-.31	0.76	-.31	0.65	.00	-
Study 3	63	31	32	-.28	0.51	-.20	0.55	.08	-
Study 4	70	37	33	5.30	1.79	5.27	1.79	-.01	.05
Study 5	101	53	48	5.75	1.84	4.96	2.22	-.19	.07
Study 6	65	33	32	4.53	1.80	4.56	1.63	.01	.38**
Study 7	93	50	43	-.17	0.62	-.52	0.78	-.25*	.07

Note. Included *N* = participants included in the analysis; CG = control group; EG = experimental group; DV = value of dependent variable in respective study. Higher (positive) values indicate higher right-wing preference; Main effect & Interaction effect = Transformed Pearson correlation effect sizes for main effect and interaction prime x subjective socioeconomic status. Dashes indicate that no data was obtained for the respective cell.
*$p < .05$. **$p < .01$. ***$p < .001$.

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No Replication, no Effect?

What Can We Conclude From Non-significant Replications?

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Abstract

Replicating previous findings is a cornerstone of science, however many replication attempts fail to reproduce significant results. The present report highlights the relevance of the largely theoretical discussions on the correct interpretation of non-significant replications and possible pitfalls of interpretation: In a first step we show how researchers and readers of the psychological literature tend to overinterpret the evidential value from non-significant findings. We then contrast these common misconceptions by demonstrating with reported replication examples and unreported 'file-drawer' studies that non-significant findings can meta-analytically provide even more evidence in favor of an effect and should be therefore assessed more carefully. We conclude with a discussion on how to interpret the cumulated data and a plea to think and test more meta-analytically.

Introduction

Every introductory methods course teaches that replications are crucial for establishing external validity because single findings are unreliable. Recently, in the wake of what has been referred to as a crisis of confidence the call for replication studies has grown louder and there is a widely shared consensus that replications of former findings are essential for a cumulative science (e.g., Cesario, 2014; Kahneman, 2012; Simons, 2014). However, even though psychologists constantly produce internal replications of their own findings and the newly established interest in reproducibility initiated a growing number of replication studies (e.g., Doyen, Klein, Pichon, & Cleeremans, 2012; Galak, LeBoeuf, Nelson, & Simmons, 2012; Gomes & McCullough, 2015; OpenScienceCollaboration, 2015; Van Dessel, De Houwer, Roets, & Gast, 2016), not as much attention has been directed on how to thoroughly interpret replications that turn out to be non-significant. Most often, it seems that non-significance is understood as a failure to replicate a former finding (Schmidt & Oh, in press; Simonsohn, 2015), which leads researchers to question their hypothesis or the validity of the original findings (e.g., Kunert, 2016, May 24; Pashler & Harris, 2012; Srivastava, 2016, February 12; Yong, 2016, March 4). As we will outline next, this conclusion is not always justified.

Overview

In the present report, we highlight the practical relevance of the largely theoretical discussions among statisticians and methodologists about the interpretation of non-significant replications. We do not claim to have developed a new method. Nor do we want to pretend to have reinvented statistical significance testing (see Cohen, 1994). But, over the last months, we have experienced in numerous exchanges on social media, formal debates, and informal conversations that some statistical truisms may not be fully ingrained within social and

experimental psychologists. We want to raise awareness for an oftentimes shortsighted interpretation of non-significant replications, outline its detrimental consequences and show how cumulative meta-analysis (CMA; Braver, Thoemmes, & Rosenthal, 2014) can change it.

In a first step, we exemplify misbeliefs about the evidential value of non-significant replications among researchers and consumers of psychological research and introduce the logic of CMA. In a second step, we focus on the evaluation of such 'failed' replications. We reanalyze data from recently published non-significant replication studies and unreported non-significant findings using CMA. Our analyses illustrate how non-significant replications can reduce as well as increase the overall evidence for an effect and therefore do not necessarily provide evidence for the null hypothesis. In a third step, we discuss the evaluation of the complete data of original studies and replications and - based on the cumulative evidence - whether an effect is empirically supported or not.

Unwarranted Conclusions From 'Failed' Replications

For decades researchers have called attention to the fact that replications should not be interpreted in isolation, have discussed the risk of potential misinterpretations that may occur, and have pointed out that single studies often only allow inconclusive inferences about the underlying effect (e.g., Cohen, 1994; Cumming, 2008; Earp & Trafimow, 2015; Fabrigar & Wegener, 2016; Rosenthal, 1990, 1997; Schmidt, Ocasio, Hillery, & Hunter, 1985; Schmidt & Oh, in press). After all, the success of a replication, defined via statistical significance, depends to a great extent on statistical chance (Cumming, 2008; Schmidt & Hunter, 2014; Stanley & Spence, 2014). However, despite the knowledge about the unreliability of single replications and the available tools to deal with it (e.g., Hunter, Schmidt, & Jackson, 1982), most replication studies are still interpreted in isolation.

To begin with the problematic consequences from this practice, it is helpful to distinguish between one sort of replications where an original study is replicated by a different research team (we term such studies as external replications) and another sort of replications where researchers conduct a series of conceptually related studies, which aim to assess the same postulated effect (we term such studies as internal replications). In reports of external replications, the single attempts are often reported without analyzing them in the larger context of the original data (see Gil-Gómez de Liaño, Stablum, & Umiltà, 2016; Johnson, Cheung, & Donnellan, 2014; Korndörfer, Egloff, & Schmukle, 2015; Pashler, Rohrer, & Harris, 2013; Rohrer, Pashler, & Harris, 2015; Wortman, Donnellan, & Lucas, 2014), which can lead to a limited interpretation of the effect. Similarly, original researchers may be led astray, because they also tend to analyze and interpret their conceptually related studies only in isolation (Goh, Hall, & Rosenthal, 2016). This can then imply that the non-significant results are either deemed as 'not worthy for publication' by authors, editors and reviewers or that it makes authors doubt their former findings. Consequently, by evaluating each study in isolation, these researchers might either create a file drawer bias (Rosenthal, 1979) or, equally concerning, might relinquish promising research projects due to non-significant findings (Fiedler, Kutzner, & Krueger, 2012).

The isolated consideration of replications is particularly worrisome because some beliefs about non-significance are flawed and can have severe consequences for the theoretical interpretation of replication results. One common belief claims "non-significance means no effect" (Maxwell, Lau, & Howard, 2015; Schmidt & Hunter, 1997). Even though we know from the general logic of Neyman-Pearson significance testing that non-significance does not indicate that the null hypothesis (H0) can be accepted just because the alternative hypothesis (H1) is rejected (Cohen, 1994), the conclusion that a non-significant result provides evidence for the H0 is tempting. However, apart from the possibility that an effect is basically zero,

non-significance can also indicate that an effect is simply smaller than previously assumed. In fact, a single replication barely provides sufficient evidence to confirm the absence of an effect (Maxwell et al., 2015). In other words, a non-significant finding usually does not mean that the true effect is essentially zero. It simply tells that we found no significant evidence that the effect exists.

Another common belief states that „non-significance questions the existence of an effect". This belief is more appropriate because it acknowledges the possibility of both, no effect and a smaller effect. According to this logic, researchers often follow from a non-significant replication to question the theoretically suggested effect (i.e. usually H1; see Gomes & McCullough, 2015; Harris, Coburn, Rohrer, & Pashler, 2013; Rohrer et al., 2015). Although this would be correct if we had only the replication data, it is not necessarily the correct conclusion when we take the original data into account. The counterintuitive notion, that a non-significant replication does not necessarily justify skepticism about an effect, becomes clearer as we will demonstrate that the integration of non-significant replications into the analysis can make the evidence for a theoretical claim even stronger.

A non-significant replication is a necessary but not a sufficient condition to demonstrate a null effect (i.e. accept H0) or even to shed doubts on a set of previous findings (i.e. reject H1). A smaller effect is usually still possible. Consequently a single replication can hardly disprove an original result or a theory but may rather add information about the reliability of the original effect (Simons, 2014).

Evidence for unwarranted conclusions from non-significant replications

In contrast to the statistical logic that non-significance does usually not allow to confirm the absence of an effect (Cohen, 1994) an analysis of 1999 volumes of the Journal of Applied Psychology and the British Journal of Psychology showed that flawed conclusions about non-

significance are widespread (Finch, Cumming, & Thomason, 2001). A stunning 37% of all non-significant findings were interpreted by their authors as providing evidence for a null effect instead of acknowledging the possibility of a smaller effect. The current replication literature sometimes mirrors similar misconceptions. The rationale that a hypothesized effect is presumably non-existent because of one or more failed replications is often a prominent interpretation of the data. By suggesting that the original findings were probably Type I errors and the underlying effect not real, the possibility of an existing but smaller effect is easily ignored (e.g., Gil-Gómez de Liaño et al., 2016; Pashler et al., 2013; Ritchie, Wiseman, & French, 2012).

Hence, if authors and reviewers sometimes tend to neglect the possibility of smaller effects in favor of the conclusion that an effect is not real, it would not be surprising if the recipients of the psychological literature including the future researchers of the field make the same mistake. In an exploratory study, we tested 105 German Psychology graduate students (18 males, 4 other) to examine their beliefs about replications (for details see Appendix A & B). In an initial question, we first wanted to assess students' general attitude on whether they believed in the benefits of replications or whether they were rather skeptic regarding the usefulness of replications. Ninety-five percent stated that the field needs either more direct (38.2%) or conceptual (56.9%) replications rather than spending more resources in the discovery of new research (4.9%), thus confirming a generally positive attitude towards replication research. In the subsequent questions, we confronted the students with scenarios of replication reports to assess which conclusions students were willing to approve.

In the first scenario, the majority of our participants indicated that a single non-significant replication that confronts one significant original finding does not allow any conclusions about the effect (88%), rather than believing in the absence of an effect (11%) or in its existence (1%), $\chi 2\ (2) = 140.97$, $p < .001$. Although the majority correctly indicated that the

data appears inconclusive considering one failed replication, only a minority resisted to draw strong conclusions about the absence of an effect when faced with more than one failed replication. After reading an abstract of a replication paper (Pashler et al., 2013) that reported three failed replication attempts of one original study, only one third indicated that one should not draw conclusions from these findings (33%). In fact, while only 2% of the students still expressed belief in the effect, the vast majority of students (65%) assumed that the original effect was presumably a false-positive finding and that the effect probably does not exist ($\chi 2(2) = 62.23$, $p < .001$) thus neglecting the possibility of a smaller effect.[1] In a subsequent task, we assessed in more detail, which causal explanations students were willing to draw from a set of inconclusive studies. To provide a detailed scenario, we presented effect sizes, sample sizes and p-values of an original study and three non-significant replications and offered four possible conclusions in a multiple choice task allowing for multiple answers (Figure 1). The four studies allowed no clear-cut conclusion about whether the effect exists or not and the purpose of the scenario was not to test whether students would provide the correct answer (which we did not offer as a response alternative). Instead we wanted to explore to what extent they were willing to question the general validity of the original study and more specifically whether they were ready to infer questionable research practices (see Simmons, Nelson, & Simonsohn, 2011). Fifty-eight percent indicated that the original study was probably a false-positive finding while 13% believed that the replications had been false-negatives. Perhaps more worrisome, 48% were ready to believe that the original researchers had engaged in questionable research practices (compared to 15% on behalf of the replicators).

[1] Ceteris paribus, the replications by Pashler et al. (2013) did of course question the reliability of the effect at first sight, but it is unlikely that even three replications provide sufficient evidence to demonstrate that the underlying effect is basically zero. Such conclusions require tremendous sample sizes (Maxwell et al., 2015).

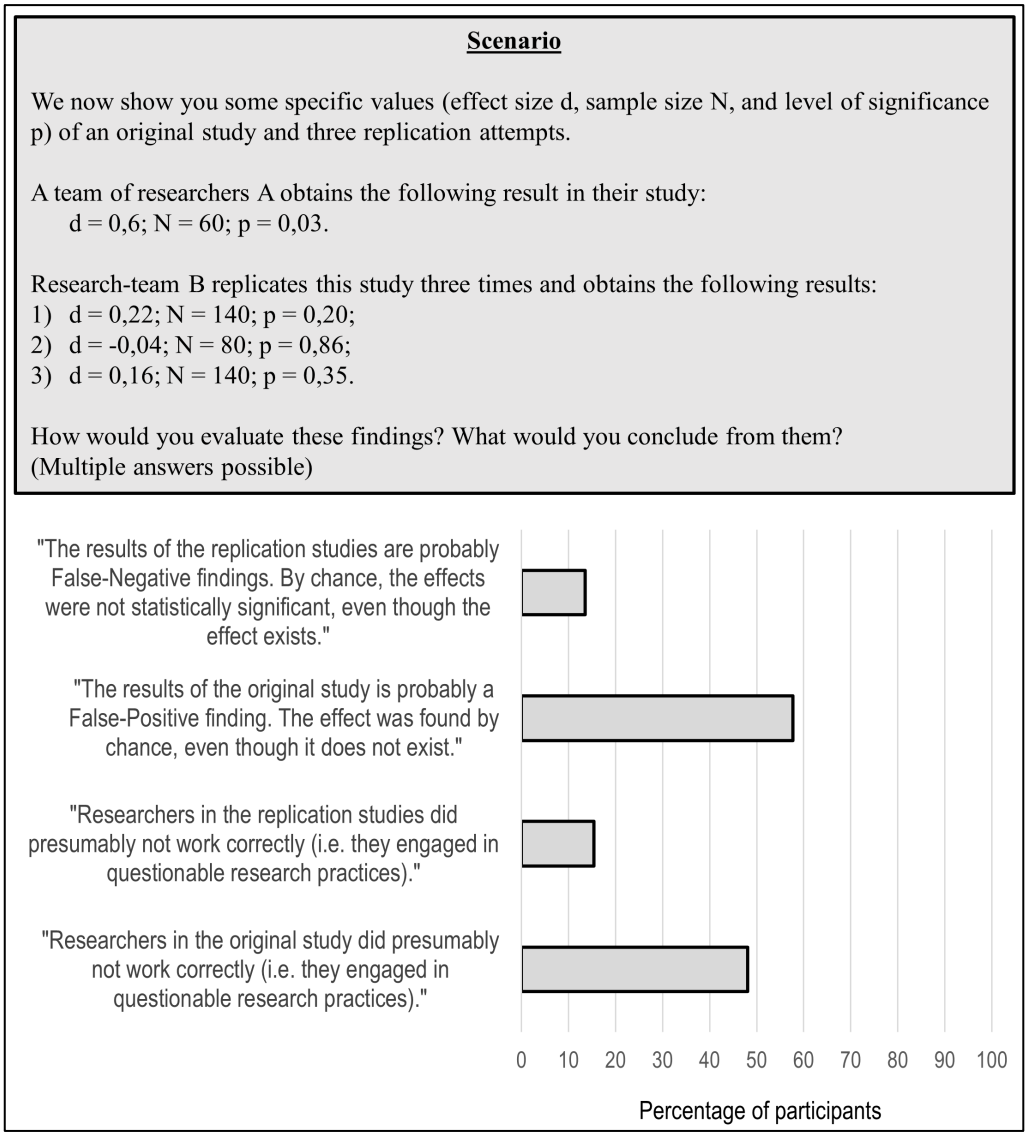

Figure 1. Grey box shows the description of a scenario with one original study and three non-significant replications and their corresponding statistics. Participants could check each of the four provided answers above. *Percentage of participants* indicates the ratio of approval ratings for each answer. A random-effects meta-analysis across the exemplary data revealed a marginally significant and homogenous effect, $N = 420$, $g = 0.20$, 95% CI = [-0.01, 0.42], Z = 1.91, $p = .056$, $Q(3) = 3.52$, $p = .32$, $I^2 = 14.70\%$. The results do not prove that the effect does not exist but rather indicate that the effect is probably much smaller and a bigger sample would be needed to provide evidence for the effect. A small effect ($d > .30$) is as possible as is an essential null effect (d < .10; see Rosenthal & Rubin, 1994).

Our findings tentatively suggest that a subgroup of readers of the psychological literature tends to overinterpret non-significant replications. In case of doubt, they disregard the possibility of a smaller effect and rather conclude from a set of mixed findings that an effect is not real. Disconcertingly, it also seems that an inconclusive but certainly expectable set of findings (see Cumming, 2012) can have the power to smear the reputation of the original researchers (and also to a much lesser extent of the replicators).

The Informational Value of Failed Replications

To answer the question what we can properly conclude from non-significant replications, we first have to define what we want to test. Usually, rather than testing whether a specific observed effect size replicates in a second experiment, we want to test the predictions of a theory. This is important, because psychology research usually seeks to test theories with ordinal hypotheses (e.g., less helpfulness, more frustration, more abstract construal level) that do not predict specific effect sizes. The size of an effect may be important for practitioners and may help other researchers to conduct future research, but it does not contain theoretically germane information for an ordinal hypothesis. Accordingly, psychologists are interested in theories – not in effects – and as long as an effect is not of irrelevant size (e.g., $-0.1 < d < 0.1$; see Maxwell et al., 2015), most theoretical assumptions are supported if the effect points in the predicted direction.

Hence, the critical question is how well single replication studies can test ordinal hypotheses. The obtained effect size in a study varies greatly due to sampling error (Cumming, 2012). If the effect size in an original finding (e.g., $d = 0.8$) were an unbiased estimate of the true underlying effect, half of the replications with sample sizes based on that previously found effect size (see Faul, Erdfelder, Buchner, & Lang, 2009) would be sufficiently powered and the other half somewhat underpowered to detect the respective

effect. However, it appears that several factors lead to systematically inflated effect sizes among the published literature even when an effect is genuine (Etz & Vandekerckhove, 2016; Francis, 2012b; OpenScienceCollaboration, 2015). Because significant findings have a better chance to be published, across different researchers who test a specific theory, those with significant p-values and stronger effects will be more likely to publish their results. But also within one team of researchers, those studies, which produced significant results and stronger effects, are more likely to end up in the literature. Hence, replications that are based on such potentially inflated effect sizes (e.g., $d = 0.8$) are prone to be underpowered to test for the underlying, smaller effect (e.g., d = 0.4; Etz & Vandekerckhove, 2016; Simonsohn, 2015; Simonsohn, Nelson, & Simmons, 2014). Because we can only guess the true size of the underlying effect, it is helpful to reason about the required sample size for the smallest effect to accept the alternative hypothesis. If we wanted to test (t-test, alpha level = .05, two-tailed, Power = .80) whether an effect exists or is essentially non-existent (e.g., $-0.10 < d < .10$), the necessary sample size ($N = 3428$) is surprisingly high (see Maxwell et al., 2015). In short, if we cannot be sure about the true underlying size of an effect, we cannot be sure whether the single replication was simply underpowered to detect a smaller genuine effect or whether the effect is indeed negligibly small. Single replications that are based on former effect sizes therefore do not reliably test an ordinal hypothesis, even if the replication's design and sample is adequately set to test the original hypothesis (ceteris paribus assumption).

Cumulative Meta-Analysis (CMA)

When lacking power to find an effect, a straightforward solution is to increase the statistical power for a test by combining the data of the available studies in a CMA. The logic of a CMA assumes that every new study is an additional piece of evidence that can speak in favor or against the existence of an effect. This approach has several benefits. First, it

facilitates the evaluation of a new replication, second, it allows an evaluation of the entire cumulated data and third, it helps to detect biases in the original or replication data.

By adding a new replication to the existing data, the CMA reveals whether the replication decreases or increases the evidence for a hypothesized effect and therefore assesses the result of a replication given the previous findings. Although in practice non-significant replications usually indicate a smaller effect-size than previously reported, they can provide more rather than less empirical evidence for an effect by increasing the meta-analytic test statistic and therefore decrease the likelihood of a Type I error (Fabrigar & Wegener, 2016). Thus a paradoxical but commonly unrecognized consequence of non-significant replications is evidenced by this approach. Adding a non-significant replication can even boost rather than diminish trust in the existence of an effect when analyzed in a CMA (Braver et al., 2014).

As a second benefit and under the assumption that neither the original studies nor the replications suffer from systematic biases, a CMA is a more powerful tool to test the common ordinal hypotheses (i.e. effects of unspecified size) compared to the analysis of single studies.[2] By including more data than a single study, the CMA decreases the confidence interval for the estimated effect size and predicts more accurately whether an effect is essentially zero or theoretically relevant (e.g., Borenstein, Hedges, Higgins, & Rothstein, 2009; Cumming, 2012). As a consequence, a small underlying effect in a research project with several studies (i.e. an unbiased set of internal replications) is much more likely to become statistically significant in a CMA than when evaluating each study individually. Moreover, if an effect is essentially zero (e.g., $-0.10 < d < .10$), CMA will quickly reduce the

[2] Note, the integration of several studies in a meta-analysis might not yield a sufficiently large sample size to provide the power to detect small effects either, but it provides the highest power available to reject or accept the H1 without gathering more data.

range of plausible effect sizes up to an extent that can provide convincing evidence for the H0 (Goh et al., 2016).

Non-significant external replications, however, often raise suspicion that the original findings have been biased. If so, a CMA is still helpful, and a better approach than an isolated interpretation of the replication results. Effect size heterogeneity in a random-effects meta-analysis (for details see: Borenstein, Hedges, Higgins, & Rothstein, 2010) indicates to which extent a set of studies reflects the same underlying effect or not (Borenstein et al., 2009). Accordingly, if a reported effect is entirely based on publication bias (i.e. an essential null effect), a random-effects CMA will likely reflect heterogeneity among the original effects and the effects from the replication studies. When a heterogeneity analysis suggests different underlying effects (for an overview on different criteria see Borenstein et al., 2009), one should be cautious to interpret the cumulated effect size as an appropriate estimate (Braver et al., 2014). Moreover, - to the extent that one can trust in the quality of the independent replication studies - one may justifiably be more skeptical about the existence of the effect when there is considerable heterogeneity and the replication studies render an effect size around zero.

Hence, while a non-significant replication study alone only informs about the statistical failure to replicate the considered effect-size, the CMA offers a broader scope and tells what most researchers want to know: Does my replication decrease or increase the statistical evidence for the effect and does that change my conclusion about whether the hypothesized effect exists or not?

Does My Replication Decrease or Increase the Statistical Evidence for an Effect?

To demonstrate how a replication alters the evidence for an effect, we present examples with real replication data some of which decrease and others which increase the evidence for the effect.

Decreasing Evidence. In a replication of Study 2 by Bargh and Shalev (2012), Wortman et al. (2014) did not significantly replicate the original effect that physical coldness leads to feelings of loneliness (p = .895, Table 1, #2). While the original study showed a significant result in favor of the effect, N = 75, g = 0.60, 95% Confidence Interval (CI) = [0.14, 1.07], Z = 2.55, p = .011,[3] a random-effects meta-analysis across the original study and the replication (Table 1, #1-2) revealed a non-significant effect for the manipulation of physical coldness, N = 335, g = 0.28, 95% CI = [-0.30, 0.85], Z = 0.95, p = .345, $Q(1)$ = 4.82, p = .028, I^2 = 79.24%. Ceteris paribus, the replication substantially decreased our confidence in the effect by decreasing the test-statistic to non-significance and more empirical evidence in favor of the effect would be required.

[3] To allow for direct comparisons between the different analyses, we calculated conventional effect sizes (Hedge's g or the Odds ratio) and the corresponding Z-values throughout all analyses. Furthermore we applied random-effects models for all meta-analyses to allow the underlying effects to vary between studies.

Table 1
Overview for Original Studies and Replications With Hedges' g Effect Size

#		*N*	*g*	*Var.*	*Lower CI*	*Upper CI*
1.	Bargh & Shalev (2012; study 2)	75	0.60	0.06	0.14	1.07
2.	Wortman et al. (2014)	260	0.02	0.02	-0.23	0.26
3.	Caruso et al. (2013; Study 4)	47	0.70	0.09	0.11	1.29
4.	Caruso et al. (2013; Study 5)	63	0.62	0.07	0.11	1.12
5.	Rohrer et al. (2015; Study 4)	116	0.13	0.04	-0.23	0.50
6.	Caruso et al. (unpublished A)	86	0.14	0.05	-0.28	0.56
7.	Caruso et al. (unpublished B)	240	0.16	0.02	-0.10	0.41
8.	Caruso et al. (unpublished C)	176	0.07	0.02	-0.23	0.36
9.	Caruso et al. (unpublished D)	45	0.62	0.09	0.02	1.22

Note. Summary statistics for studies with Hedges' g effect size. *N* = combined sample size; *g* = unbiased standardized effect size; *Var.* = respective variance of the effect. *CI*-values represent the lower and upper 95% confidence intervals of the effect size of for each study.

Another example comes from research by Caruso, Vohs, Baxter, and Waytz (2013a) who found in two studies that participants primed with the concept of money showed stronger beliefs in the fairness of free markets (Table 1, #3-4). Rohrer et al. (2015) replicated these findings by Caruso et al. (2013a) and found no significant effect (p = .474, Table 1, #5). A random-effects meta-analysis across both original findings showed that U.S.-American participants[4] who were primed with images of money, reported a stronger fair-market ideology, N = 110, g = 0.65, 95% CI = [0.27, 1.04], Z =3.30, p = .001, $Q(1)$ = 0.05, p = .83, I^2 = 0.00%. A CMA across both original studies and the replication from Rohrer and

[4] Caruso and colleagues (2013) argue that US-Dollar primes only provoke stronger fair market ideologies for American citizens but not for non-Americans. Thus, similar to Rohrer et al. (2015), we only included U.S. participants throughout these studies.

colleagues (Table 1, #3-5) with more than twice as many participants revealed a smaller but still significant effect, $N = 226$, $g = 0.43$, 95% CI = [0.05, 0.81], Z = 2.23, $p = .026$, $Q(2) = 3.65$, $p = .161$, $I^2 = 46.13\%$. Although the CMA still reached conventional levels of significance, the replication reduced the meta-analytic test statistic for the effect and therefore decreased the overall evidence for the effect.

Increasing Evidence. Counterintuitively, non-significant replications can also increase the overall test statistic in a CMA. Thus, the addition of a non-significant replication can increase the evidence for an effect just like a significant replication, as the following examples illustrate. Zhong and Liljenquist (2006) investigated in their Study 3 whether recalling an unethical deed would result in a higher likelihood to take an antiseptic cleansing wipe, $N = 32$, Odds ratio *(OR)* = 1.61, 95% CI = [0.09, 3.13], Z = 2.08, $p = .038$. The replication by Gámez, Díaz, and Marrero (2011) was not significant ($p = .185$; Table 2 #2) and thus the authors reasoned that "(…) among Spanish subjects physical cleansing is not an operant conduct in order to restore moral purity." (p. 160). Although the result does not allow to conclude that physical cleaning restores moral purity among Spanish participants, it paradoxically increases the likelihood that the overall effect exists. The meta-analysis across both studies (Table 2, #1-2) suggested that their ostensibly failed replication provided additional evidence for the effect by increasing the test statistic to Z = 2.40, $p = .016$, $N = 77$, $OR = 1.31$, 95% CI = [0.24, 2.38], $Q(1) = 0.29$, $p = .589$, $I^2 = 0.00\%$. In Study 4 by Zhong and Liljenquist (2006), the authors found that physical cleansing could reduce participants' compensatory behavior (volunteering to help), when being morally threatened, $N = 45$, $OR = 1.41$, 95% CI = [0.15, 2.67], Z = 2.19, $p = .028$. Once again, the replication by Gámez et al. (2011) showed no significant effect itself ($p = .163$; Table 2, #4), but increased the cumulated evidence (Table 2, #3-4) in favor of the effect, by increasing the test statistic from Z = 2.19, $p = .028$ in the original study to Z = 2.60, $p = .009$, $N = 73$, $OR = 1.37$, 95% CI = [0.34, 2.41],

$Q(1) = 0.01$, $p = .923$, $I^2 = 0.00\%$. When considered as a piece of cumulative evidence, these two replications would not be regarded as failures to replicate the effect but rather as additional evidence for the respective overall effects.

Table 2
Overview for Original Studies and Replications With Odds Ratio Effect Size

#		*N*	*OR*	*Var.*	*Lower CI*	*Upper CI*
1.	Zhong & Liljenquist (2006; study 3)	32	1.61	0.60	0.09	3.13
2.	Gámez et al. (2011; study 3)	45	1.02	0.59	-0.49	2.53
3.	Zhong & Liljenquist (2006; study 4)	45	1.41	0.41	0.15	2.67
4.	Gámez et al. (2011; study 4)	28	1.30	0.87	-0.53	3.13

Note. Summary statistics for studies with Odds ratio effect size. *N* = combined sample size; *OR* = log Odds ratio effect size; *Var.* = respective variance of the effect. *CI*-values represent the lower and upper 95% confidence intervals of the effect size of for each study.

A third example how ostensible evidence against the existence of an effect can essentially show the opposite comes from a set of four internal replications (Caruso, Vohs, Baxter, & Waytz, 2013b; see Table 1, #8-11), which as pointed out by Rohrer and colleagues (2015) were not published in the original paper by Caruso and colleagues (2013a). Three out of four studies showed non-significant results (Table 1, #8-10). Including these three studies in addition to the two original studies (Caruso et al. 2013a) and the replication study by Rohrer and colleagues (2015), the meta-analysis (Table 1, #5-10) revealed a significant and homogeneous effect, $N = 728$, $g = 0.20$, 95% CI = [0.05, 0.35], Z = 2.68, $p = .008$, $Q(5) =$

6.37, $p = .27$, $I^2 = 0.00\%$.[5] Intuitively, one might feel tempted to interpret the unreported studies as evidence against the effect. However, similar to our previous examples, the inclusion of the three unpublished and non-significant studies provided more, rather than less evidence for the effect by increasing the test statistic from Z = 2.23, $p = .026$ to Z = 2.68, $p = .008$.

Non-significant replications that are considered a failure on an individual level can not only decrease but also increase the evidence for an often smaller but real effect on a cumulative level (for more examples see: Fabrigar & Wegener, 2016; Scheibehenne, Jamil, & Wagenmakers, 2016). However, whether the overall evidence for an effect increased or decreased does not yet answer the theoretical question of whether the effect exists. As we will argue next, the consideration of the whole data also allows a well-informed answer to this question.

Does the Effect Exist?

The question whether an hypothesized effect should be – based on the entire given data – considered true or not is fairly complex and much debate is going on to which extent one can trust published original data and how reliable new (mostly external) replications are (e.g., Francis, 2012a; Pashler & Harris, 2012; Stroebe & Strack, 2014).

In an ideal research world, a set of findings would not be biased by questionable research practices or publication bias. In such cases, one can reliably interpret the overall estimate of a CMA as a good – or at least a better – indicator for the true underlying effect than each of the single study results. It would be thus also appropriate to conclude from such an estimate

[5] A fourth unpublished finding (Table 1, #9) actually reached conventional levels of significance ($p = .041$) and was erroneously reported as a "null effect" (Rohrer et al., 2015, p. e75). A meta-analysis across the original studies, the replication and all four unpublished studies (Table 1, #3-9) also revealed a significant effect, $N = 773$, $g = 0.22$, 95% CI = [0.08, 0.37], Z = 3.08, $p = .002$, $Q(6) = 8.13$, $p = .23$, $I^2 = 0.00\%$

whether an effect is likely to exist (accepting H1) or not (rejecting H1), by using null-hypothesis significance testing or alternatively confidence intervals (Cumming, 2008) or Bayesian statistics (Scheibehenne et al., 2016). For instance, researchers who conduct several conceptually related studies can – when conducting proper analyses (Funder et al., 2013) – usually rule out the possibility of systematic biases. By integrating the studies into a CMA, those researchers will obtain a more accurate estimate and more statistical power to test their hypothesis.

However, published findings are often under the suspicion of being biased and their respective effect sizes inflated. If some of the integrated studies in a meta-analysis are indeed biased, the CMA estimate is consequently biased as well and the test statistic might reflect a significant though false-positive effect. Thus, to deal with this dilemma, it has been suggested to rely solely on external, independent replications when evaluating the existence of a hypothesized effect (Simons, 2014). Various approaches (e.g., Simonsohn, 2015; Verhagen & Wagenmakers, 2014) have been proposed that provide sounder assessments of replications than simply asking whether the single replication is statistically significant.

Of course, the reliance on external replications is a plausible idea, but it is associated with other problems. Independent replications might suffer (although possibly to a smaller extent) from systematic biases as well. For example the strength of the manipulation (Finkel, 2016), the characteristics of the sample (Dang, 2016), or the setting of the study (Wilson, 2016) might systematically decrease a replication's effect size (e.g., Cesario, 2014; Fabrigar & Wegener, 2016; OpenScienceCollaboration, 2015). Luttrell, Petty, and Xu (in press) illustrate this problem by demonstrating in an exemplary replication study that the extent, to which a replication study actually reproduced the psychological conditions in an experiment, moderates whether the study replicates the original finding or not. In other words, as for any original study, the effect obtained in a replication study might be a biased indicator for the

underlying effect as well. A second problem of the exclusive analysis of external replications is the comparably lower statistical power. Especially with only few replications at hand, discarding parts of the available data might lead to insufficient power to detect smaller effects or to provide evidence for the absence of an effect.

Thus, apart from approaches that rely solely on the replication data (assuming that replications are the only trustworthy, unbiased data), one approach is to integrate replications and original studies and check whether they differ. As advocated before, CMA provides such a tool by estimating heterogeneity in the data. Keeping the benefits from improved power, the heterogeneity index serves as an indicator to which extent the underlying effect sizes differ from each other. If the analysis indicates low levels of heterogeneity, the meta-analytic effect size provides a satisfying effect size estimate for the – so far available – data. But in case the analysis indicates substantial heterogeneity we should be more skeptic either about the replication(s) or about the original findings and the reliability of the effect.

Conclusions and Recommendations

We demonstrated the relevance of an issue that has so far only been shown from a theoretical perspective (Maxwell et al., 2015; Schmidt & Oh, in press) and with statistical simulations (Braver et al., 2014; Cumming, 2008; Fabrigar & Wegener, 2016): Non-significant replications can not only undermine but also increase the evidence for an effect and thus change the current interpretation of the data. Furthermore, we showed that the existence of unreported, non-significant studies that might raise doubt about the validity of an effect among other researchers (Francis, 2012b, 2013; Vadillo, Hardwicke, & Shanks, 2016) can also indicate the opposite. Although some skepticism is warranted, such unreported findings may often weaken but may also boost the evidence for an effect in some cases. At

least the example of non-reported findings we reported shows that a file drawer questions the reported size but not inevitably the existence an effect.

This might be viewed as old news for statisticians, methodologists and meta-analysts, but the rationale that non-significance does not inevitably indicate 'no effect' and does not even necessarily question a hypothesized effect seems to be often overlooked and possibly not yet fully ingrained among psychologists. The consequential misconceptions about non-significant results that we have outlined in this report seem to be widespread and can have detrimental consequences for original researchers, replicators and readers of the psychological literature. The over-interpretation of non-significant internal replications might lead to publication bias or ultimately to the dismissal of promising theories when too many findings turn out to be non-significant. The interpretation of a non-significant external replication as a piece of evidence for the absence of an effect – without even testing whether the replication increases the evidence for an effect on a cumulative level - might provoke unwarranted conclusions by the readers of replication reports and the replicators themselves. This should be no argument against replications in general, but it highlights the responsibility of researchers to carefully evaluate the evidence of a non-significant replication and to provide readers of replication reports with balanced and justified conclusions. One of the external replications we illustrated here (Wortman et al., 2014) gives an excellent example that many researchers already do so.

We believe CMA can help to interpret non-significant replications in two relevant contexts. First, significance testing of single external replication studies is not a convincing method to estimate the existence of an underlying effect because it is highly unreliable. Researchers should evaluate and report their replications in a CMA at least complementary to their single analysis, to illustrate how the new replication changes the overall evidence for the effect (Donnellan, Lucas, & Cesario, 2015; Gomes & McCullough, 2015; OpenScienceCollaboration, 2015; Pecher, van Mierlo, Cañal-Bruland, & Zeelenberg, 2015).

If a replication increases the evidence for an effect on a meta-analytic level, it would be odd to conclude on a single-analytic level that it failed. Furthermore, a CMA analysis provides readers of replication reports with a more comprehensive picture of the different findings and can help researchers to gather enough cumulative evidence to show that an effect is negligibly small. Second, the CMA approach can also be applied for series of internal replications (e.g., Hall, Goh, Mast, & Hagedorn, 2016; Schuler & Wänke, 2016; Tuk, Zhang, & Sweldens, 2015). When considering their conceptually related experiments as cumulative evidence, researchers should neither be discouraged by single non-significant replications and abandon their theory, nor should they be tempted to ignore the data. A meta-analytic approach will not only help to meet the newly established requirements of many journals for increased power (e.g., Vazire, 2016), but will also provide a more realistic and convincing summary of one's research (for a primer see: Goh et al., 2016).

Some reservations regarding the use of meta-analyses have been articulated that might partly apply for our proposed cumulative meta-analytic approach as well. If a part of the studies in a CMA is biased, then the meta-analytic estimate will be biased too (Sakaluk, 2016). Therefore, we proposed heterogeneity analysis to discover possible differences between biased and unbiased sets of studies. Because common tests of heterogeneity (*Q*-Test, I^2 confidence interval) lack statistical power with smaller numbers of studies (Huedo-Medina, Sánchez-Meca, Marín-Martínez, & Botella, 2006), it has been suggested to raise the alpha level (Fleiss, 1993) or to consider a cut-off criterion that does not depend on the numbers of studies ($I^2 > 50\%$; Braver et al., 2014). However, it might still be that real heterogeneity is not detected in a smaller set of diverging studies. As a second limitation of using CMA, it has been voiced that researchers might engage in meta-hacking, the practice of creatively integrating studies into a meta-analysis until the desired outcome is generated (Sakaluk, 2016; Ueno, Fastrich, & Murayama, 2016). Such a practice is of course perfectly possible as is any

other questionable research practice with any other type of analyses. *Cumulative* implies the integration of all available and proper studies, but a method cannot prevent from scientific misconduct. However, we strongly doubt that meta-analysis will increase it either. Our hope is that CMA will reduce the focus on p-values and will facilitate the integration of non-significant findings into regular research reports. Such a meta-analytic research culture that takes the natural occurrence of non-significant findings into account might stimulate more sustainable findings and might ultimately help to regain confidence in psychological research.

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Appendix A

Study Description

Participants. Participants were recruited online at different universities and in the laboratory at the University of Heidelberg. We exclusively recruited German-speaking Psychology graduate students to make sure our participants are regular readers of psychological literature and possess the necessary statistical education to evaluate our questions. We predetermined our minimum sample size with at least 88 participants to arrive at a power of $1-\beta > .80$, assuming a medium effect size of $\omega = 0.3$ with $\alpha = .05$ for our critical items two, three and four (Faul, Erdfelder, Buchner, & Lang, 2009). However we recruited as many students as possible until our deadline of July 31, 2016.

Sample characteristics. From 155 participants, we excluded 27 who left no data or did not complete the study and therefore could not be identified as Psychology graduate students. We further excluded ten non-Psychologists and 13 undergraduate-Psychologists who accidently accessed the survey online. The remaining 105 participants (18 males, 4 other) were on average 25.4 years old ($SD = 3.64$). 71% of the participants reported good to very good statistical skills. 99% reported at least advanced knowledge of the German language and 93% reported at least advanced knowledge of the English language.

Language. The questionnaire was presented in German except the abstract of item 3, which was kept in its original English format.

Appendix B

Study on Interpretations of Non-significant Replications

Instructions:

Dear survey participants,

Psychological research conducts replications at increasing intervals. Thereby, past research is conducted one more time and researchers examine whether the results of the original study replicate. In the discussion on these replications, many different opinions exist. We are therefore interested about your opinion on replications in Psychology.

Items & Results:

1) What is your opinion on replications? (N = 102, 3 missings)

a)	I think more replications should be conducted, where the same materials are used as in the original study.	38.2%
b)	I think more replications should be conducted, where researchers try to replicate an effect with different materials	56.9%
c)	I think money, time and participants' course credit should be invested in new research rather than to replicate old research.	4.9%

2) Below you will find a summary of a replication study, which could not replicate the original results:

> A group of scientists tested in one study the efficacy of a new therapy against depression. Patients reported after the new therapy less depressive symptoms than patients in the control group that had been treated with a common therapy. An independent research group repeated this study and found no significant difference between the group treated with the new therapy and the group that had been treated with the common therapy.

What would you conclude from these findings? ($N = 105$)

a) Probably there is an effect of therapy: the new therapy is better than the old one. 1%

b) Probably there is no effect of therapy: the new therapy and the old one are equally efficient. 11.4%

c) Probably there is an inversed effect of therapy: the new therapy is worse than the old one. 0%

d) I would not draw any conclusions from these findings. 87.6%

3) Below you will read an abstract (summary) of a scientific article that reports a replication study.

> In a simple study involving 64 participants, Rasinski, Visser, Zagatsky, and Rickett (2005) reported that requiring people to make semantic judgments involving four words related to honesty (embedded among other words) increased the likelihood that they would later admit to having engaged in problematic alcohol-related behaviors (e.g., drinking to the point of blackout). If valid, this honesty-priming effect would offer a powerful intervention to improve the validity of self-report data in many different contexts. To determine whether the effect is repeatable, we first attempted two replications using the same materials, tasks, and measures used by Rasinski et al. Experiment 1 repeated the study with a sample of 150 students. No priming effects were observed here, nor in a follow-up study using adults recruited on the web (Experiment 2). Experiment 3 used the same priming manipulation together with a more refined measure of response candor (derived from Paulhus, 1991). Again, the honesty-related primes had no detectable effects.

How would you interpret these findings? ($N = 105$)

a) The original study by Rasinski et al. (2005) was presumably a False-Positive finding (the effect became significant by chance, even though it doesn't exist). The effect itself does probably not exist. 64.8%

b) The three replication studies were presumably False-Negative findings (the replications did not become significant, even though the effect exists). The effect itself probably still exists. 1.9%

c) I would not draw any conclusions about the assumed effect from these findings. 33.3%

4) We now show you some specific values (effect size d, sample size N, and level of significance p) of an original study and three replication attempts.

> A team of researchers A obtains the following result in their study:
> d = 0,6; N = 60; p = 0,03.
>
> Research-team B replicates this study three times and obtains the following results:
>
> 1) d = 0,22; N = 140; p = 0,20
> 2) d = -0,04; N = 80; p = 0,86
> 3) d = 0,16; N = 140; p = 0,35

How would you evaluate these findings? What would you conclude from them? (Multiple answers possible, $N = 105$)

a)	The results of the original study is probably a False-Positive finding. The effect was found by chance, even though it does not exist.	58.1%
b)	The results of the replication studies are probably False-Negative findings. By chance, the effects were not statistically significant, even though the effect exists.	13.3%
c)	Researchers in the original study did presumably not work correctly (i.e. they engaged in questionable research practices).	47.6%
d)	Researchers in the replication studies did presumably not work correctly (i.e. they engaged in questionable research practices).	15.2%

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The effect of money priming on self-other focus

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Abstract

Theoretical considerations underlying the effects of money priming assume that priming individuals with money makes them focus more strongly on themselves than on others. However, recent research did not replicate original findings. Moreover, past research has not yet directly tested the assumed underlying mechanisms and has used one or just a few items to measure the effects of money priming. Thus, more systematic research is needed to shed light onto the effects of money priming. The aim of the present research was to administer a robust and highly reliable task that allows testing self-other focus on a trial-by-trial basis. More specifically, we tested in two studies the effect of money priming on self-other focus in the imitation-inhibition task. In line with theoretical considerations underlying the effects of money priming, Study 1 found an increased focus on the self as compared to others when primed with money. In Study 2 we aim at adjusting the method of Study 1 and running a highly powered preregistered experiment based on a small effect size.

Keywords: money priming, automatic imitation, self-other focus

Since its beginning eleven years ago, money-priming research has generated a wide range of surprising findings (Gasiorowska, Zaleskiewicz, & Wygrab, 2012; Hansen, Kutzner, & Wänke, 2013; Kouchaki, Smith-Crowe, Brief, & Sousa, 2013; Quoidbach, Dunn, Petrides, & Mikolajczak, 2010; Teng, Chen, Poon, Zhang, & Jiang, 2016; Tong, Zheng, & Zhao, 2013; Yang et al., 2013) that led to a large number of publications (for review see: Vohs, 2015). In their theoretical approach, Vohs, Mead, and Goode (2006) assume that beside other driving mechanisms subtle money related stimuli heighten the accessibility of the concept of money without participants' conscious awareness. The activation of this concept leads to a market pricing orientation (Fiske, 1991) in which actions and options are evaluated in terms of personal inputs and outputs. This general orientation is accompanied by an inner state of *self-sufficiency* "wherein people put forth effort to attain personal goals and prefer to be separate from others" (Vohs et al., 2006, p. 1154). That is, "people become more self-oriented when reminded of money and thus shift their focus more toward self-related needs and less toward the needs of other people" (Reutner & Wänke, 2013, p. 220). As a consequence, this psychological state causes a general focus on the self increasing personal goal pursuits, personal benefits, and personal performance, as well as decreasing the desire to interact with others (Schuler & Wänke, 2016; Vohs et al., 2006; Vohs, Mead, & Goode, 2008).

These theoretical considerations have been supported in a series of findings. For example, when being primed with money, participants show an increased focus on one's own benefits and goals (Gąsiorowska & Hełka, 2012; Reutner & Wänke, 2013; Teng et al., 2016) and put more emphasis on the achievement of personal tasks (Boucher & Kofos, 2012; Zedelius, Veling, & Aarts, 2013). Moreover, research has shown that the emphasis on the self comes along with less concern about others. For

instance, it has been found that subjects reminded of money are less inclined to interact with others (Vohs et al., 2006), to help others (e.g., Guéguen & Jacob, 2013; Vohs et al., 2006) and to consider other persons' needs (Gino & Mogilner, 2014; Kouchaki et al., 2013).

Despite this diverse evidence, research on money priming has recently undergone heavy criticism. Several money priming studies did not replicate as expected (Klein et al., 2014; Rohrer, Pashler, & Harris, 2015; Schuler & Wänke, 2016) and two articles became suspected (one convicted) of data fraud (cf. Pashler, Rohrer, Abramson, Wolfson, & Harris, 2016; Retraction, 2014). Even more troubling, a meta-analysis by Vadillo, Hardwicke, and Shanks (2016) suggests that the seminal findings on money priming that inspired the entire field (i.e., Vohs et al., 2006) might be biased by selective reporting and other questionable research practices.

Therefore, more systematic research is needed to test one of the core assumptions underlying money priming—namely the claim that money priming leads individuals to focus more strongly on themselves as compared to others (e.g., Vohs et al., 2008). Previous research on money priming has not yet directly tested this suggested underlying mechanism and many of the assessed behavioral measures are based on one or just a few items, which goes in line with low internal reliability (cf., Epstein, 1980). Moreover, most studies used explicit measures that may be prone to demand effects. In the present study we aimed at testing whether money primes lead to an increased focus on the self (as compared to others) with the imitation-inhibition task (Brass, Bekkering, Wohlschläger, & Prinz, 2000; Stürmer, Aschersleben, & Prinz, 2000)—an established, unobtrusive, robust, and highly reliable trial-by-trial task that measures self-focus via automatic imitation. Indeed, in a recent study, Genschow et al. (in preparation) found in an highly powered study (N = 200) a split-

half reliability of α = .86. Moreover, a recent meta-analysis on the imitation-inhibition task yielded an effect size of g_z = 1.03 for reaction times indicating a very strong imitation effect (Cracco et al., under review).

Automatic imitation

Individuals imitate a wide range of different behaviors including facial expressions (Dimberg, 1982), postures (LaFrance, 1982), gestures (Bernieri, 1988), or simple movements (Brass et al., 2000; Genschow & Florack, 2014; Genschow, Florack, & Wänke, 2013; Genschow & Schindler, 2016; Hofree, Urgen, Winkielman, & Saygin, 2015)—to name just a few examples. According to ideomotor theory (e.g., Greenwald, 1970; Prinz, 1990, 1997), these imitative tendencies are the result of shared perception-action codes in the brain. In detail, ideomotor theory assumes that observed actions trigger an imitative response because they activate a representation that contains not only visual but also motor information. An important consequence of this process is that there is an intrinsic overlap between self- and other-representations (e.g., Lamm, Bukowski, & Silani, 2016).

A prominent task to measure imitation is the imitation-inhibition task (Brass, Bekkering, & Prinz, 2001; Brass et al., 2000). In this task, participants respond to a number while a hand on the screen either performs a congruent or incongruent movement. A congruent movement speeds up responses and an incongruent movement slows down responses. The difference in response speed between incongruent and congruent trials is the imitation-inhibition congruency effect. According to a prominent theoretical view, this congruency effect can be seen as a measure of self-other focus (Brass, Ruby, & Spengler, 2009). In support of this theory, research has shown that an increased focus on the self as compared to others

reduces automatic imitation (Hogeveen & Obhi, 2011; Leighton, Bird, Charman, & Heyes, 2008; Leighton, Bird, Orsini, & Heyes, 2010; Spengler, Brass, Kühn, & Schütz-Bosbach, 2010; van Baaren, Maddux, Chartrand, De Bouter, & van Knippenberg, 2003; Wang & Hamilton, 2013). For instance, Hogeveen and Obhi (2011) primed participants with words related to an interdependent self-construal or independent self-construal and found smaller congruency effects during independent as compared to interdependent primes. Relatedly, Spengler and colleagues (2010) found reduced congruency effects when increasing participants' self-focus by letting them sit in front of a mirror during the imitation-inhibition task. In sum, the imitation-inhibition task measures the degree to which individuals focus on their own, as compared to someone else's motor plan, and can thus be used as an implicit measure[1] of self-other focus (e.g., Brass et al., 2009; Spengler et al., 2010).

Present research

Taken together, theoretical assumptions underlying many money priming findings put forward the idea that priming individuals with money makes them focus more on themselves than on others. Previous research supporting this claim has been heavily criticized, has not directly tested the underlying mechanisms, and has often used explicit measures with just few items that are prone to demand effects and low reliability. Hence, more systematic research regarding the theoretical claims of money priming is needed.

The aim of the present research was to administer a robust, unobtrusive and highly reliable paradigm that allows testing self-other focus on a trial-by-trial basis in the motor domain. Within the imitation-inhibition task we hypothesized smaller congruency effects during trials that contain money priming as compared to neutral

trials. In order to test this hypothesis, we first ran a pilot study. Based on the results of the pilot study we adapted our paradigm and then ran a highly powered preregistered experiment based on estimated power to detect a small effect (i.e., $d = .30$).

Pilot study

Method

Participants and design. In return for partial course credit, 47 students from Ghent University (Belgium) participated in this study. All participants reported normal or corrected-to-normal vision. Previous to analysis we excluded 5 participants: Two participants were excluded because the response box did not detect any response and three participants were excluded because they committed errors in more than 50% of the trials suggesting random responses. The final sample contained 42 participants (12 males) with an age ranging from 18 to 27 ($M = 21.90$; $SD = 2.97$). The design consisted of a 2 (congruency: congruent trials vs. incongruent trials) x 2 (priming: money vs. neutral) within-subject design.

Stimuli and apparatus. The imitation-inhibition task (Brass et al., 2001, 2000) was programmed using Tscope5 software (Stevens, Lammertyn, Verbruggen, & Vandierendonck, 2006) and was run on Asus Eee PC 1215N laptops with an external 17-inch Dell monitor. Responses were recorded with a custom-built response box, which used light sensors to detect lifting movements of participants' index and middle fingers. Stimuli consisted of different frames (523 x 422 pixels). The frames depicted a female left hand and were positioned in order to create a mirror image of the participant's right hand (i.e., the response hand). To produce an illusion of movement, the hand was first presented in a neutral resting position and was then overwritten by a second picture of the hand in the final position. The final position depicted the model either having the index finger or the middle finger lifted.

Depending on condition, the model's hand was either lying on a bunch of money bills (i.e. 100€, 20€, 10€ bills) or on paper sheets that contained the exact same pixels as the money bills, but were regrouped in random order.

Procedure. The pilot study was conducted at the end of a series of other experiments. After participants signed a written informed consent, the experimenter gave some general oral instructions on the imitation-inhibition task. Participants then ran through 8 practice trials to get familiar with the task. Afterwards, participants performed two blocks of the imitation-inhibition task. One block consisted of money stimuli and one block of neutral stimuli. The reason of using such a blocked manipulation instead of a trial-by-trial approach was to reduce the possibility of carry over effects. Moreover, to further work against carry over effects we introduced a short break between the two blocks. The order of the experimental blocks was counterbalanced across participants and each block consisted of 120 trials. After the experiment participants were fully debriefed and dismissed.

Imitation-inhibition task. Within the imitation-inhibition task, participants had to lift their index or middle finger in response to a letter (A = lift index finger, B = lift middle finger) on the computer screen while watching a congruent or incongruent finger movement (cf. Figure 1). Incongruent trials and congruent trials were presented randomly. Each trial started with the appearance of a picture showing a female hand mirroring participants' right hand in resting position for 500 ms. Afterwards, a second picture of the model lifting either the index or middle finger was presented for 2000 ms or until participants responded. Simultaneously the imperative cue (i.e. letter A or B) was displayed between the model's index and middle finger. Between trials, the screen remained blank for 1000 ms.

Data preparation. To prepare data for analysis, we excluded excessively fast and slow trials. That is, trials with a reaction time (RT) faster than 100 ms (0.02%) and trials with a RT slower than 1000 ms (0.63%) were excluded (cf. Catmur & Heyes, 2011; Cracco, De Coster, Andres, & Brass, 2015). For the RT analyses, erroneous trials (7.42%) were excluded as well.

Results

Latencies. We hypothesized smaller congruency effects within money priming trials as compared to neutral trials. To test this hypothesis we conducted a 2 (congruency: congruent trials vs. incongruent trials) x 2 (priming: money vs. neutral) ANOVA for repeated measures. The results yielded a main effect for congruency, $F(1, 41) = 128.37$, $p < .001$, $dz = 1.75$, indicating that participants responded faster in congruent trials ($M = 410.68$; $SD = 23.97$) than in incongruent trials ($M = 447.32$; $SD = 43.85$). The main effect for priming was not significant, $F(1, 41) = 0.28$, $p = .60$. More important for our predictions, however, was the significant interaction between congruency and priming, $F(1, 41) = 4.09$, $p = .050$, $dz = .31$, indicating smaller congruency effects in the money priming condition ($M = 40.58$; $SD = 23.12$) than in the neutral condition ($M = 32.69$; $SD = 25.76$). The results are depicted in Figure 2.

Error rates. In additional analyses we analyzed the error rates of the imitation-inhibition task with a 2 (congruency: congruent trials vs. incongruent trials) x 2 (priming: money vs. neutral) ANOVA for repeated measures. The ANOVA yielded a main effect for congruency, $F(1, 41) = 32.57$, $p < .001$, $dz = .88$, indicating that participants committed more errors in incongruent trials ($M = 8.18$ %, $SD = 5.31$) as compared to congruent trials ($M = 3.69$ %, $SD = 2.90$). Neither the main effect for priming, nor the interaction between congruency and priming was significant, $F(1, 41) < 1.18$, $p > .284$.

Additional Analyses. In two additional 2 (block order: neutral first vs. money first) x 2 (priming: money vs. neutral) mixed ANOVAs with block order as between subject factor, priming as within subject factor and the congruency effect as dependent variable we tested whether there were any order effects. The interaction between block order and priming was neither significant for latencies, $F(1, 40) = 1.60$, $p = .214$, nor for error rates, $F(1, 40) = 0.05$, $p = .817$. Similarly, neither for latencies, $F(1, 40) = 2.42$, $p = .128$, nor for error rates, $F(1, 40) = 0.22$, $p = .639$, there was a significant main effect of block order. These results suggest that there was no order effect.

Discussion

By assessing the imitation-inhibition task (Brass et al., 2001; 2000), the results of the pilot study showed a smaller congruency effect for the money priming condition as compared to the neutral condition, thereby providing first support for the hypothesis that money priming increases the focus on the self as compared to others on a basic perceptual-motor level. These results are in line with Vohs et al.'s (2008) theoretical account assuming that money priming leads individuals to focus more strongly on themselves as compared to others. However, three critical issues need to be further addressed in order to draw any conclusions.

First, based on previous research suggesting that people reminded of money shift into work mentality (Vohs, 2015), one may argue that money priming increased participants' motivation to be good at the task at hand and that increased motivation rather than a focus on the self drove our effects. Indeed, theoretical accounts of motivation suggest a strong link between motivation and cognitive control (Carver & Scheier, 2001; Kruglanski et al., 2002; Simon, 1967)—a process that is involved in the imitation-inhibition task as well. Thus, one may argue that increased working

mentality may potentially explain the interaction between congruency and priming. However, it is important to note that a person's motivation is based on the activation and representation of specific cognitive and behavioral goals (Kruglanski et al., 2002). As the goal in the imitation-inhibition task is to respond as fast as possible to the target letters on the screen, the increased working mentality hypothesis would have predicted a main effect of priming. That is, the more participants should be motivated to be good in the task, the faster they should respond to the letters on the screen. As this was not the case in our experiment, we regard the alternative explanation of increased working mentality as rather unlikely.

Second, it might be that attentional processes triggered our effect. That is, money priming may have grasped participants' attention so participants did not focus on the other person's hand anymore. However, we do not regard this alternative explanation as plausible, because participants' task was to respond to the letters on the screen and not to the observed movements. Therefore, if money would have grasped participants' attention, one should have expected slower responses and more errors in the money condition as compared to the neutral condition for both congruent and incongruent trials. This, however, was not the case indicating that the money prime did not grasp more attention than the control prime. Nevertheless, it might still be that money distracted participants' attention from the hand, but not necessarily from the target letter. The random assembly of pixels in our neutral condition might, thus, not have been the ideal control condition.

Third, recently, it is suggested that "researchers themselves must provide direct replications of their own effects" in order to deal with the unreliability of a single study (Cesario, 2014). Due to the natural fluctuation of *p*-values in significance testing, every study is prone to Type I and Type II and might therefore yield

unreliable results (Cumming, 2008). In other words, empirical findings might reflect empirical artifacts rather than the underlying truth because the statistical results vary as a function of measurement error and sampling error (see Stanley & Spence, 2014). However, the likelihood to obtain a finding that reflects the underlying truth can be increased through larger sample sizes and measures with higher reliability. To minimize measurement error in our pilot study we used already a highly reliable trial-by-trial measure instead of a one-trial behavioral measure. With 120 repeated trials, we decreased measurement error to a minimum and increased the instrument's reliability.

However, it is important to note that despite decreasing sampling error in our pilot study, our finding might still be a false-positive. Moreover, although we controlled for color information in the neutral condition, it might still be that the money condition guided participants' attention away from the model's hand decreasing the congruency effect. Thus, to cope with these problems, we will run a preregistered study in which we change the neutral condition and adjust the a-priori power based on a previously set effect size. Moreover, a meta-analysis was carried out to cross-validate our observed effects. Specifically, we ran a meta-analysis on all previous published experiments that used manipulations related to self-other focus and the imitation-inhibition task as dependent variable.

Preregistered experiment

Based on the results of the pilot study we expect smaller congruency effects in the money priming condition as compared to the control condition. In order to test this hypothesis we will conduct a highly powered preregistered experiment. Before data

collection, we will preregister the experiment (i.e., exact method, design, data preparation and data analysis) at the Open Science Framework (OSF; https://osf.io/).

Method

Participants and design. In order to estimate the sample size, we conducted a power analysis using G*Power 3.1 (Faul, Erdfelder, Lang, & Buchner, 2007). We aimed at detecting a small but still reasonable effect size with high power given practical limitations (Lakens, 2014). This resulted in an effect size of $d = 0.30$, which is similarly high to the effect detected in our pilot study ($dz = 0.31$). When using G*Power to estimate the sample size to detect an effect with an effect size of $dz = .30$ within a dependent t-test (two-tailed), a power of $1- \beta = .95$ and an alpha error probability of $\alpha = .05$, the optimal sample size is N = 147 participants.

Participants will be students from the University of Cologne (Germany) and will be recruited for a reaction time study in return for a payment of 5€. In line with the pilot study, the design of the preregistered experiment consists of a 2 (congruency: congruent trials vs. incongruent trials) x 2 (priming: money vs. neutral) within-subject design.

Stimuli and apparatus. Although the preregistered study is very similar to the pilot study, they differ in some details. First, a difference concerns the device to detect participants' responses. As the participant number is rather high and our lab does not have more than two custom-built response boxes with light sensors, we plan to apply a measure that can be more easily used. That is, we plan to use keyboards to detect participants' responses. Therefore, at the beginning of each trial, participants will be asked to press down the "G" key with their right index finger and the "H" key with their right middle finger. In line with the pilot study, participants are instructed to lift their index finger in response to the letter A and their middle finger in response

to the letter B displayed on the screen. The use of keyboards to measure automatic-imitation has been found to be a reliable measure within many different studies (e.g., Aicken, Wilson, Williams, & Mon-Williams, 2007; Boyer, Longo, & Bertenthal, 2012; Butler, Ward, & Ramsey, 2015; Santiesteban, Banissy, Catmur, & Bird, 2015; Sowden, Koehne, Catmur, Dziobek, & Bird, 2015).

Second, we will use different stimuli. That is, instead of a hand that is perceived as clearly female, we will use a neutral hand. Furthermore, although the stimuli in the money condition look similar to the pilot study, we changed the stimuli in the neutral condition in line with previously used money priming stimuli (e.g., Reutner & Wänke, 2013) to rule out the assumption that attention drives the effect. Specifically, we chose pictures of a model's hand that is placed on colored paper sheets that match the colors of the money bills. Moreover, in order to load the paper sheets with a similar amount of information we printed symbols (i.e., §, #, *) and objects (i.e., house, bridge, tower) on the paper sheets. In total, we will us two different backgrounds of the model's hand. That is, depending on condition, the hand will either lie on money bills or on colored paper sheets.

Procedure. The procedure will be similar to the pilot study. First, participants sign an informed consent. Second, participants will run through the imitation-inhibition task. There will be no actual cover story introduced. Instead, participants will read specific instructions about the imitation-inhibition task. That is, they are instructed to respond as fast and as accurate as possible to the letters "A" and "B" on the screen (A = lift index finger; B = lift middle finger). In order to get familiar with the task, participants will then run through 8 practice trials. Afterwards, participants will perform two blocks of the imitation-inhibition task. One block will consist of money stimuli and one block of neutral stimuli. Between these two blocks,

participants are allowed to take a small break. The order of the experimental blocks will be counterbalanced across participants and each block consists of 120 trials (i.e., 60 congruent and 60 incongruent trials). The total amount of experimental trials is 240. Finally, participants indicate basic demographic data — namely gender and age — will be fully debriefed by the experimenter and dismissed.

Imitation-inhibition task. In line with the pilot study, participants will lift their right index or middle finger in response to a letter (A = lift index finger, B = lift middle finger) on the computer screen while watching a congruent or incongruent finger movement. Incongruent trials and congruent trials will be presented randomly. Each trial will start with the appearance of a picture showing a neutral hand mirroring participants' right hand in resting position for 500 ms. Afterwards, a second picture of the model lifting either the index or middle finger will be presented for 2000 ms or until participants respond. Simultaneously the imperative cue (i.e. letter A or B) will be displayed between the model's index and middle finger. Between trials, the screen will be blank for 1000 ms.

Data preparation. In line with the pilot study, we will exclude participants for which no responses are detected and participants who commit errors in more than 50 % of the trials. For each excluded participant, we will test another participant in order to ensure the pre-registered sample size. Moreover, in line with the pilot study and with past research on the same task (cf. Catmur & Heyes, 2011; Cracco et al., 2015) we will exclude trials with RT faster than 100 ms and RT slower than 1000 ms in order to prepare data for analysis. For the RT analyses, erroneous trials will be excluded as well.

Data analysis

In line with the pilot study we will report *F* tests and d_z as measure of effect size in order to allow comparison and cross-referencing with the results of the pilot study.

Latencies. To test our predictions we will conduct a 2 (congruency: congruent trials vs. incongruent trials) x 2 (priming: money vs. neutral) repeated measure ANOVA. We will report main effects as well as the interaction.

Error rates. The same analyses that have been conducted for the latencies will be run for the error rates.

Cross-validation with previous findings. In order to cross-validate our findings we will descriptively compare the detected effect size in the preregistered study with the average effect size of previously published experiments that used manipulations related to self-other focus and the imitation-inhibition task as dependent variable. In a first step we ran a meta-analysis. Specifically, we included all published papers in which the imitation-inhibition task was performed by an adult sample after self-focus priming, neutral priming, or both (Cook & Bird, 2011; Cook & Bird, 2012; Hogeveen & Obhi, 2011; Leighton et al., 2010; Spengler et al., 2010; Wang & Hamilton, 2013; Wang & Hamilton, 2015). Since research has shown that the effect of self-other focus reverses when it is primed from a third-person (Wang & Hamilton, 2013; Wang & Hamilton, 2015), we did not include self-focus effect sizes that used third-person primes. This procedure resulted in a sample of ten experiments from seven papers that were included in the meta-analysis. From these ten experiments we obtained ten self-focus priming effect sizes ($N = 155$) and five neutral priming effect sizes ($N = 101$).

For three experiments, we were able to extract all relevant information from the paper (Cook & Bird, 2011; Cook & Bird, 2012; Hogeveen & Obhi, 2011). For

another three experiments, we could calculate effect sizes on the basis of the raw data (Wang & Hamilton, 2013). Finally, the effect sizes of the final four experiments were extracted from the graphs using a computerized measurement (Leighton et al., 2010; Spengler et al., 2010; Wang & Hamilton, 2015). Because four experiments provided multiple effect sizes from the same sample of participants (e.g., within-subject manipulation), we used robust variance estimation with correlated effects weights and a small sample correction to account for the fact that these effect sizes were correlated (Tanner-Smith, Tipton, & Polanin, 2016). A mixed effects model with a moderator for condition revealed a significant difference between the congruency effect after self-focus ($d_z = 0.85$) and control ($d_z = 1.43$) priming, $t(5.37) = 3.75$, $p = .012$ (Figure 3).[2]

In order to cross-validate the effects observed in the preregistered experiment with previous findings that used a similar methodological approach, we will descriptively compare the effect size of the congruency effect in the money priming condition with the meta-analytic effect size for self-focus (i.e. $d_z = 0.85$) and the effect in the control condition with the meta-analytic effect sizes for control primes (i.e. $d_z = 1.43$).

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Footnotes

[1] It is important to note that participants' goal is to respond as fast and accurate as possible to the numbers on the screen and not to respond towards the hand on the screen or to engage in the same goal as the model's hand. Thus, the congruency effect measures self-other focus in rather implicit way.

[2] Even though third-person primes revealed reverse effects for self-other focus, we repeated the meta-analysis without excluding third-person primes for interested readers. This analysis revealed that the difference between the self-focus (d_z = 1.00) and control (d_z = 1.44) congruency effect was close to significance, $t(5.03) = 2.33$, p = .067.

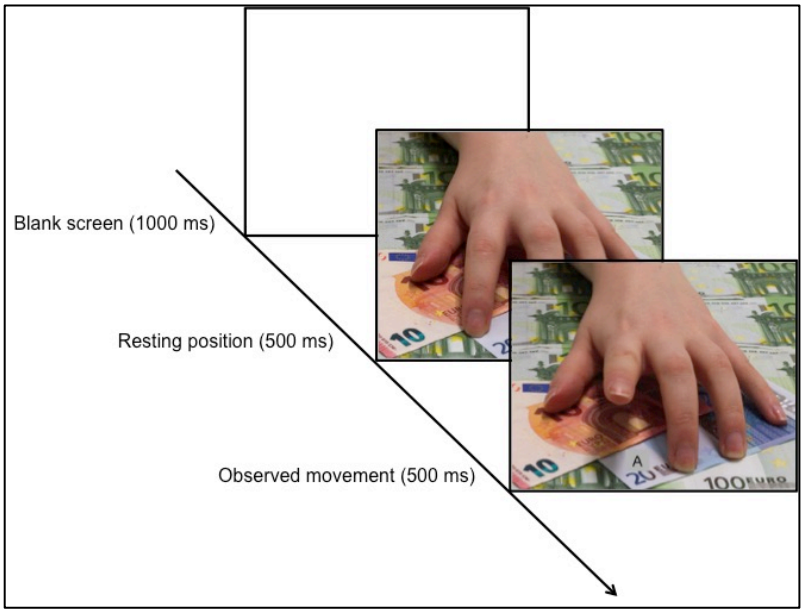

Figure 1. Procedure of a trial in the imitation-inhibition task.

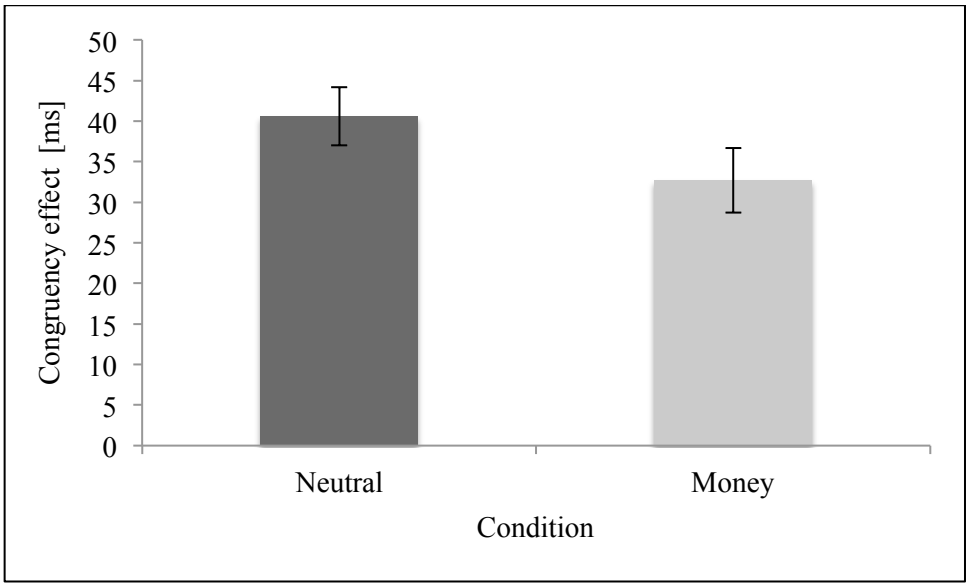

Figure 2. Congruency effect as a function of priming. Error bars represent standard errors of the mean.

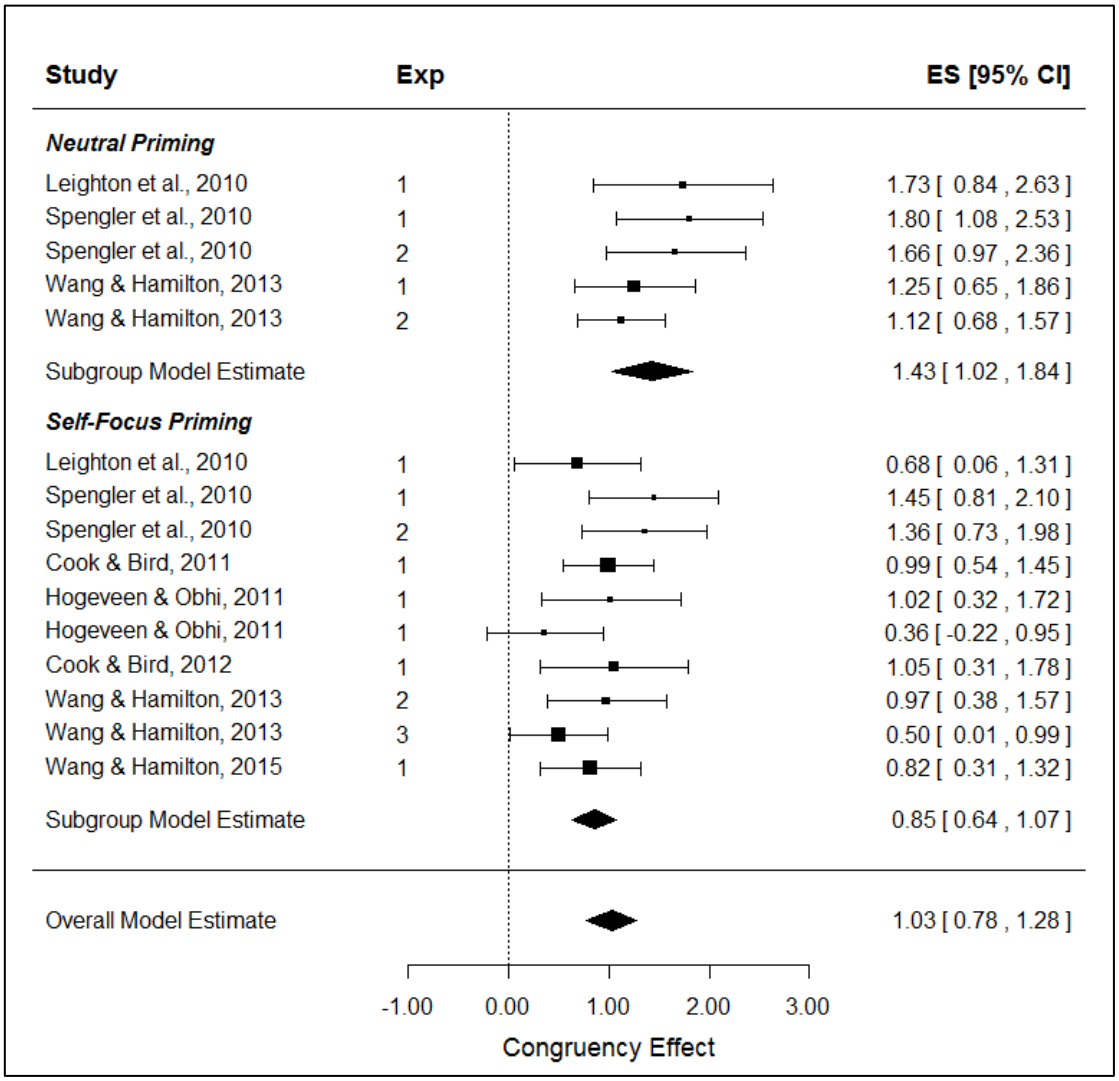

Figure 3. Forest plot for the relation between the reaction time based congruency effect in the imitation-inhibition task (Brass et al., 2002, 2000) and self-other focus.